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LAWSON TAIT

HIS LIFE AND WORK

W. J. STEWART McKAY

LAWSON TAIT

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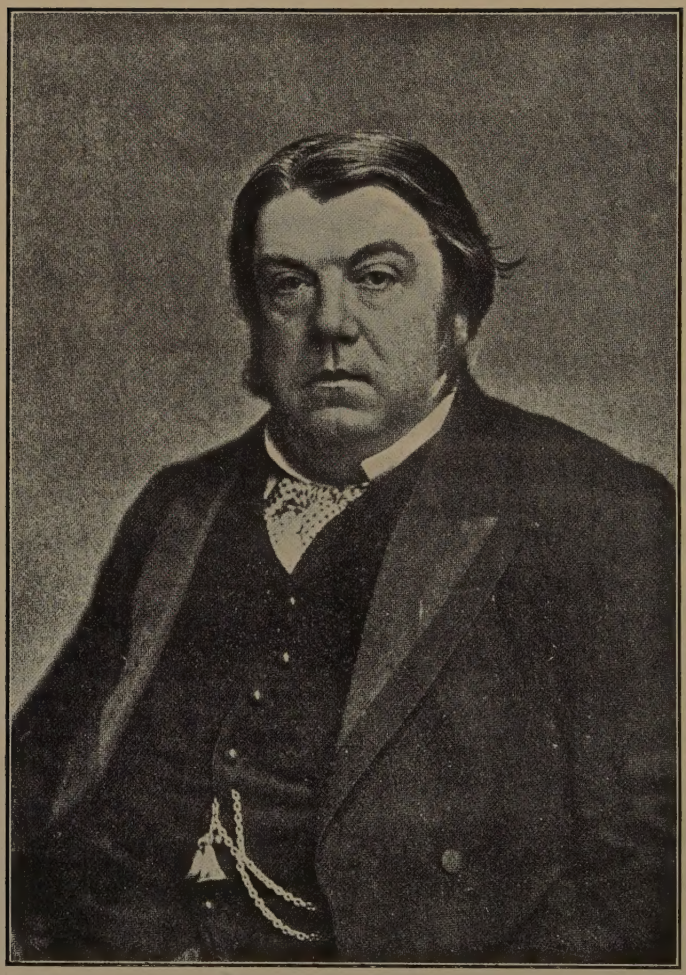
LAWSON TAIT'S PERINEAL OPERATIONS.
With a Preface by LAWSON TAIT.

THE HISTORY OF ANCIENT GYNÆCOLOGY.

THE PREPARATION AND AFTER-TREATMENT OF SECTION CASES.

OPERATIONS UPON THE UTERUS, PERINEUM, AND ROUND LIGAMENTS.

PLATE I.



LAWSON TAIT.

This photograph was published in the *Lancet* of 24th June, 1899, at the time of his death, and is here reproduced by kind permission of the proprietors.

Frontispiece.

LAWSON TAIT

HIS LIFE AND WORK

A CONTRIBUTION TO THE HISTORY OF ABDOMINAL SURGERY AND GYNÆCOLOGY

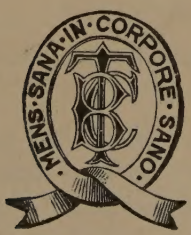
BY

W. J. STEWART McKAY

M.B., M.Ch., B.Sc.

"Since death has closed his career, let us accept of the genius that is gone, the rich and complex inheritance it has left us"

SAINTE-BEUVE



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1922

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FOREWORD

"He was a remarkable man. In some ways he was a giant, and the power of the man held you like a vice. As a surgeon he was a genius; justice has not yet been done to him, and possibly never will be, for the part he took in that wonderful development of abdominal surgery which occurred in the last quarter of the nineteenth century."

JORDAN LLOYD.

Address on Surgery, 79th Annual Meeting of the B.M.A., Birmingham, July, 1911.

"The cavities of the body were a sealed book until the father of modern abdominal surgery, Lawson Tait, and our own Joseph Price, carried the sense of sight into the abdominal cavity."

WILLIAM J. MAYO, M.D.

St. Paul's Medical Journal, November, 1914.

"In Pelvic Surgery Tait stood first, and taught us the best we know. He has had no very close second; his disciples have not greatly improved upon his ways."

JOSEPH PRICE.

PREFACE

To give an outline of the life and work of Lawson Tait, to record some of the steps in the evolution of Gynæcology, and, lastly, to show that Tait was the founder of modern Abdominal Surgery, are my reasons for writing this book.

I have divided it into two parts. In the first part an endeavour has been made to trace briefly what was known about any subject before Tait specially dealt with it in order to show what he contributed, by his individual efforts, to our sum of surgical knowledge.

In the second portion of this work, a detailed account of Tait's method of operating has been given, and something is told of his private life; this part being founded on personal observations made by me when acting as his assistant at Birmingham in 1891.

The plural of the personal pronoun has been used in Part I., while in Part II. the singular of the pronoun has been employed as being more suitable for the personal tone of the narrative.

Lawson Tait was a great personality, and those who practise abdominal surgery and gynæcology are under great obligations to "that great pioneer in our art," as Howard Kelly and E. Hudson, in their splendid work on the Appendix, call him; while William Mayo has styled him "the father of modern abdominal surgery."

But as the years pass by we are inclined to forget our obligations to the dead, and, as Alexander Miles has pointed out, the student of surgery who confines his reading to modern text-books fails to see the outlines of his subjects in their true historical perspective, and, while the existing state of surgery as a science and an art is duly presented to

him, he neglects to trace the steps which have led to its present-day efficiency. He is content to view the perfected results of surgery, and does not realise the difficulties that were encountered in the past by the pioneers, whose names only linger because they are attached to a particular operation or a particular instrument.

If I can bring home to the present generation what my great master did for abdominal surgery, I shall be content. I have little doubt that a grateful posterity will one day acclaim him the *Founder of Modern Abdominal Surgery*.

STEWART McKAY.

SYDNEY,
NEW SOUTH WALES.
June, 1922.

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PART I

I

Edinburgh

1845–1866

Robert Lawson Tait was born at 45, Frederick Street, Edinburgh, on May 1, 1845. He was the son of Archibald Campbell Tait, of Dryden, who was a Guild Brother of Heriot's Hospital, and, according to Alban Doran, was a lawyer and cousin to the Archbishop of Canterbury.

His mother was Isabella Stewart Lawson of Leven.

Heriot's Hospital is an institution which was founded by the Jeweller of James the Sixth of Scotland, and at his death, in 1624, he left his estate in trust to the Magistrates and Ministers of Edinburgh for the maintenance and education of poor fatherless sons of freemen of the city.

Tait received his primary education at this institution, and at that period some 180 boys were maintained on the foundation, 120 of these were resident, and 60 non-resident. Those among the scholars who gave promise of diligence and ability were afterwards maintained during the full course of four years at the University. Tait showed the necessary promise, and having won a scholarship, entered the University of Edinburgh in 1860. He began the Arts Course, but he abandoned this after his first year, for he says in one place :

> I remember very well the first case of death that I witnessed from menstrual hemorrhage, due to myoma, occurred in Dr. Mathews Duncan's own practice while I was a pupil at the Edinburgh Infirmary in 1862. The case was impressed on my memory because I made a post mortem examination of the patient and carried the tumour to Dr. Duncan.[1]

---

[1] "Diseases of the Ovaries," 4th edit., 1883, p. 325.

Dr. Charles Read, of Cincinnati, Ohio, in an able memorial address [1] on Tait's Life and Work, says :

> We are informed that he did not graduate at this latter institution (University of Edinburgh), although he studied in the departments of both the Arts and Medicine. Those who were students with him in this great institution remember him as a lad of great aptitude, but with comparatively little continuity of application. He was fond of those diversions which appealed with much force to vigorous young manhood. As a consequence he found prescribed tasks very irksome and the imposed discipline very annoying. He is reported as having been more intimately acquainted with the proctor than with any other officer of the institution. But his expanding and virile mind could be no less active than his vigorous body. The themes which engaged his attention, however, were those which occurred out of the ordinary routine. The great work of the Darwins was then creating a storm centre in the scientific, to say nothing of the religious, world ; and Huxley and Spencer were commanding alike the anathemas of the ecclesiastics and the plaudits of reactionary thinkers. It was to this latter class that young Tait belonged, and continued to belong during his whole life. His devotion to the new philosophy brought him into antagonism with the orthodox thought that then dominated, as it yet dominates, this great seat of learning. It was in these controversies that he developed and displayed much of that polemic tendency and forensic power, which in subsequent years proved at once his strength and his weakness. But the nature of the subject that constituted the basis of these early controversies was such as to direct his attention to the more rational methods of study and investigation. Following the lead of Darwin, he studied things rather than words. There never was a time that he was not more attracted by an object itself, than by the most artistic description of it. He early acquired the habit of translating words into ultimate conceptions, a habit which in subsequent years made him a severe and formidable critic for loose thinkers and careless writers.
>
> It was precisely this preliminary training that fitted young Tait for the most successful prosecution of his medical studies. His pupilage in the medical school was in the essentially didactic era of medical teaching. Learned lectures, prepared entirely with reference to doctrine and dictum, were the dominant features of instruction. The laboratories, with their heartless iconoclasm, scarcely yet

[1] *Amer. Journ. Obst.*, 1889, xl. p. 840.

PLATE II.

TAIT, 1855, READING A BOOK.

To face page 4.

had a footing. Personal authority was not yet dethroned. The dictum of the professor was yet accepted over and above demonstrated facts of contrary significance. Against this order of things, young Tait, with great love of truth, but with no profound reverence for men, naturally, found himself in revolt. In the medical school he was attracted and influenced by men who were already breaking away from the thraldom of tradition. Over and above the others of the class was the illustrious Sir James Y. Simpson, to whom the subject of our sketch sustained the relation of a special pupil, and later that of assistant.

Tait, in speaking to us of his Edinburgh days, mentioned some men to whom he owed much. One of these was McKenzie Edwards, who was the favourite pupil of Sir William Ferguson, and a young surgeon of great ability and prominence, and who was well known as a rapid and brilliant operator. One thing Tait learnt from McKenzie Edwards, and that was his method of opening into the abdominal cavity, for, writing to the *Lancet*, May 21, 1898, he says:

> I can neither confirm nor correct Mr. Mayo Robson's statement that Mr. Greig Smith approved of the principle of exposing the kidney without cutting muscular fibres, but as I taught him to open the abdomen by splitting open the muscular sheath and separating the fibres of the *rectus*, I have no doubt he also approved of, and practised, what I also taught him, to open the loin similarly, whether operating on the kidney or the colon by what Mr. Mayo Robson believes is a new method. If its history is a matter of importance, then I ought to put it on record that I was taught it in Edinburgh by the late McKenzie Edwards as the best way to imitate a sphincter in colotomy, thirty-five years ago, and that I have practised it uniformly ever since, and with an incision, I should say, about one-third the size indicated by Mr. Mayo Robson.

In another place in the *Lancet*, June 25, 1898, he says, in speaking about Lumbar colotomy:

> The operation as I give it I got from the lips of Alexander McKenzie Edwards, lecturer on surgery in the Extra-Mural School of Edinburgh, to whom I was apprentice, pupil, and assistant for about six years. . . . He was a surgical genius of the highest order, as many can testify who are still alive who knew him, so that I have no doubt it was a conception

of his own, and if any name is to be attached to it, his name alone ought to be.

Another was Sir Henry Littlejohn, whose assistant he was for a time. The others whom he always mentioned with admiration and enthusiasm were Syme and Simpson.

Syme at the time that Tait came to Edinburgh was regarded not only as a great clinical teacher, but also as one of the greatest surgeons in the world. He was one of the boldest and most original operators of the era which immediately preceded the introduction of anæsthesia. As early as 1823, when he was only twenty-four years of age, he performed the first amputation of the hip joint which had been done in Scotland; four years later he successfully removed the lower jaw, and in 1829 he excised the upper jaw for the first time in Great Britain. He divided the sterno-mastoid muscle for the relief of wry-neck, and claimed that this subcutaneous tenotomy was the first recorded in British surgery. His great operation on the ankle joint, which bears his name, was introduced in 1846; and his operation on aneurism displayed his unrivalled qualities as a surgeon, for he was accustomed to open the sac, insert his finger, stop the bleeding from within, and then pass the ligature around the artery above and below.

Syme had been appointed to the Chair of Clinical Surgery of the University in 1833, and he held the post for thirty-six years. His excellence as a teacher consisted in the soundness and directness of his instruction, while the sureness of his surgery raised it into the highest region of Art.

Tait considered Syme the greatest operator that he had ever seen, and Marion Sims,[1] who visited Edinburgh in 1862, wrote:

> I saw a great deal of Dr. Syme, and saw him operate repeatedly. I have seen all over the world great surgeons operate, in my country, in London, and in Paris, but I have never seen such an operator as Dr. Syme. He was a man of remarkable diagnostic powers, infallible judgment, and was wonderfully rapid and precise in execution.

---

[1] "The Story of My Life," J. Marion Sims, 1885, p. 509.

PLATE III.

TAIT AS A STUDENT AT EDINBURGH, 1862.

To face page 6.

Syme was always very ready for controversy,[1] and he drove his point well home when he got the chance. While he was cute and sagacious, he was at the same time very obstinate, and though a man of great talent, he had no genius; he was a very different stamp of man to Simpson. In his home life he was very hospitable, and at Millbank, his country house, he was accustomed to entertain all the distinguished medical men who came to Edinburgh to see Simpson and himself.

Tait was a frequent visitor at Millbank, and here he often met Dr. John Brown, author of "Rab and his Friends." This exquisite dog story, which is said to have been composed at one sitting, won for John Brown a warm corner in the heart of Tait, always a lover of animals.

Tait and Syme had one subject in which they both took much interest, and that was Botany. Tait had given much time to the study of Biology and was a keen botanist, investigating with his own microscope the lower forms of the vegetable world. Syme was also a great botanist. When he first purchased Millbank, in 1840, he found it a comparatively unsightly spot, but time, money, and taste soon improved, until, as Gross[2] says, after visiting them in 1869, "it became a paradise." John Brown, in his sketch of Syme's last days, says:

> I was the first to see him struck down by hemiplegia. It was in Shandwick Place where he had his chambers, sleeping and enjoying his evenings at beautiful Millbank with its flowers, its matchless orchids, and heaths and azaleas, its bananas and grapes and peaches. Blackford Hill, where Marmion saw the Scottish host mustering for Flodden in the front, and the Pentlands, with Clarkelton Hill, their advance guard, cutting the sky, its ruddy porphyry scaur holding the slanting shadows in its bosom.

It was from this same house that Lister had been married to Syme's eldest daughter, in 1856.

With Simpson, Tait was much more intimate, and, while

[1] "Syme and Simpson, so often in opposite camps, were still fighting the duels, airing their protestations in the daily press, and opposing each other in the courts of law." Logan Turner: "Sir William Turner," p. 62.

[2] "Autobiography," vol. i. p. 288.

still a student in the medical school, used to assist Simpson. This fact is made plain from a letter in the *Lancet*,[1] written by Tait in 1870, where he says in speaking of the operation of extirpation of the coccyx, " It is more than eight years since I assisted Sir James Simpson to extirpate the bones of the coccyx in a case of inveterate coccygodynia." That would place the operation about 1862, when Tait was a first or second year student. We may, therefore, ask : " How was Tait so fortunate as to come into such intimate relations with the most remarkable and famous medical man of the day ? " The answer is, that the very general opinion at one time was that Tait was the natural son of Simpson. We had not heard this story when we first went to Birmingham in 1891. One evening when sitting with Tait on his house-boat on the Avon, we were speaking about Simpson, and casually asked him what sort of a man Simpson was in appearance. Tait leaned forward, and taking a lock of his hair he pulled it down on his forehead and said, " Now you see him." He then told us that it was said that he was Simpson's natural son. He remarked that he did not know whether it was true or not, but he gave us the impression that he was not at all displeased with the idea.

Some months later when in Edinburgh, we were walking along one of the principal streets, and looking up, we saw a statue. At the first glance we thought it was Tait, but on crossing over and reading the inscription we found it was Simpson.

Gross who visited Edinburgh in 1868 saw Simpson, and he has left this sketch of him :

> In his appearance Simpson was as extraordinary as he was in his social and professional relations. He was of medium stature with an enormous head, well covered with flowing locks, a light complexion, bright blue eyes, a well-shaped mouth, a short thick neck, and broad shoulders, the whole set off by a countenance indicative of benevolence and intelligence.

Laing Gordon, in his life of Simpson, thus paints him :

> The striking form with which nature had endowed him became more remarkable when affected by years of work

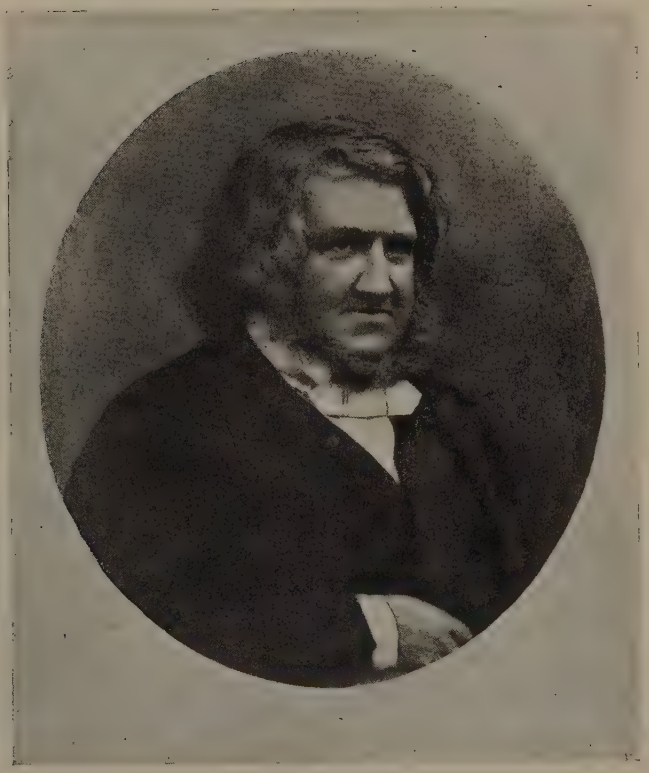
[1] The *Lancet*, May 14, 1870, p. 714.

PLATE IV.

LAWSON TAIT. 1891.

To face Plate V.

PLATE V.



SIR JAMES Y. SIMPSON.

From H. Laing Gordon's "Life of Simpson" ("Masters of Medicine" Series).

To face page 8.

> and domestic affections. Though of medium height, his presence even beside typical large-boned and large-built fellow countrymen was never insignificant. His features, overhung by his massive forehead, and surrounded by the long thick hair, spoke his character. The firm concentrated mouth and piercing eyes, when his mind was fixed on a scientific or practical object. The soft womanly tenderness about his lips, and the general sympathetic emotion in his deep-set eyes when aroused by an object of pity or pleasure. His hand was broad and powerful, but the fingers were pointed and had a specially sensitive touch.

This description of Simpson paints Tait very accurately, if we except the pointed fingers. Tait's hands were short and broad, and his fingers were anything but pointed; one could hardly imagine a more unsuitable-looking hand for a surgeon, but when you saw that hand execute the work of the master brain you forgot its plebeian look, and admired its magical dexterity.

Our own conclusions, after seeing various pictures of Simpson, and especially after seeing his statue in Edinburgh, where he is represented as a short, broad-chested, bulky man, were, that the suggestion put forward about Tait's relationship to Simpson had the most remarkable physical proof to support it, while Tait's extraordinary mental power can be easily accounted for if we accept Simpson as his father.[1]

Tait told us that he lived in Sir James Simpson's house, Queen's Street, Edinburgh, and was Simpson's assistant, and it was probably during this period that he saw Simpson cure ulcer of the bladder by establishing a vesico-vaginal fistula. In writing to the *Lancet* in 1870 on this subject, Tait says: "I venture to refer to two cases under his treatment in which I was associated with him, Simpson, and as we came one day from her bedside, he reasoned to the effect that the indication in her case was to put the bladder in a state of complete physiological rest."

---

[1] Tait used to wear a 7½ hat, while the following result of the post-mortem examination of Simpson's head shows its great size. Skull: circumference round by occipital protuberance and below frontal eminence, 22½ inches; from ear to ear, 13 inches; from occipital protuberance to point between superciliary ridges, 13 inches. The weight of the entire brain (cerebrum and cerebellum), 54 ounces.

Tait was more than fortunate in being privileged to have this intimate association with Simpson, who was regarded not only by the Scotch people, but by people from all parts of the world, as the most famous medical man alive.

Syme was a man of talent, but he was without a particle of genius, and without the least infusion of that *mens divinior* which at once explained the popularity of Goodsir, and in a remarkable degree, that of Simpson. A writer at the time of Simpson's death says: "Since the days of Sir Walter Scott we doubt if there was a Scotchman who commanded so much hearty and loyal affection from all grades of people."

Tait, in describing Simpson's everyday life, said that he was a man who never tired. His house in Edinburgh was more like an hotel than a private house, and hither flocked patients from every part of the civilised world. Gross, when visiting Edinburgh in 1868, said that Simpson invited him and his wife to breakfast, luncheon, dinner, and tea.

> On these occasions there were always some remarkable and intelligent people at table; sometimes distinguished strangers from a great distance, or from foreign countries. The morning before we left he had at breakfast, to which he had kindly invited us, two Egyptian doctors, who came purposely to Edinburgh to form his acquaintance. In fact, at his house there was a constant round of unostentatious hospitality. Sir James always had at command a fund of interesting anecdotes, and every guest felt quite at his ease. As a talker he was one of the most charming of men, always brimful of accounts of great personages, ghosts, murders, and Church affairs. He seemed to be at home upon every subject. His house was a perfect caravansary, open at all times to everybody. . . . The house was spacious and well arranged . . . several rooms were set apart for the reception and examination of patients, who came to Sir James from all parts of the country, the Continent of Europe, the United States, and even the Antipodes. Several hours in the morning and afternoon were generally spent in his consulting-room, where he was aided by one of his more advanced pupils, and a well-trained female nurse.

Among the well-known men that Tait would see in Edinburgh, and study under, were Miller, for a number of years a Professor of Surgery, and Spencer, who later held the same chair; Joseph Bell, the great-grandson of the

celebrated Benjamin Bell; Christison, the authority on poison; Hughes Bennet, and Annandale, who later on became Professor of Surgery.

While Tait may not have met Lister, yet he knew him by sight, for Lister, after acting as Syme's house surgeon and assistant from 1855, left Edinburgh to go to Glasgow in 1860 to take the Chair of Surgery, vacant through the death of Laurie, and he did not return permanently to Edinburgh until 1869, when he came to take Syme's chair. During the time that Tait was at Edinburgh, however, Lister sometimes came there to assist at one of Syme's big operations. This is made clear by a passage in one of Tait's papers, "The Evolution of the Aseptic Method in Surgery," where he says: "But look at his [Syme's] gigantic enterprises, which often in my recollection brought Lister from Glasgow, Bickersteth from Liverpool, to have the pleasure of assisting."

A few months before his death Tait wrote a letter to the *British Medical Journal* on "The Causa Causans of Typhus Fever." In this letter he says:

> I was born in Edinburgh in 1845, at a time when it was known as "the fever-stricken city," and its vast educational establishments were threatened with ruin on account of this "fivver," as typhus was known by the inhabitants. As a student I had close personal acquaintance with it, and if I had been asked then as to its causa causans I should have replied, "Living in the West Port, St. Mary's Wynd, or the Meat Market Close," the experienced officers of the old infirmary could guess with fair accuracy the particular place of the origin of each case by the severity of its symptoms. As a clerk in the old fever ward, No. VI., I never dated my day's diary without coming away with a splitting headache, which was cured by a spin round Arthur's seat. In 1861 eight of us were laid down with the plague, and, if my memory serves me rightly, only two of us got up: I know the death-roll was a heavy one, so the disease has ever interested me.

Tait qualified as L.R.C.S. and L.R.C.P., Edinburgh, in 1866, and after contributing some papers to the medical journals at this period, he left Edinburgh and visited Dublin and other Schools of Medicine, and in 1867 was appointed House Surgeon to the Wakefield Hospital.

Let us follow him to Wakefield.

II

Wakefield and Ovariotomy

1867–1870

In 1867 Tait accepted the position of House Surgeon to the Wakefield Hospital. Wakefield was a market town in the West Riding of Yorkshire, situated on the river Calder, and at this period had a population of about 26,000. It was somewhat old-fashioned in appearance, but the streets were spacious, and the town enjoyed the advantage of an atmosphere less polluted by smoke than other of the large towns of Yorkshire, such as Leeds, which was but nine miles away.

Tait always looked back to his days at Wakefield with pleasure, for it was here that he met Miss Sibil Stewart—the daughter of a solicitor of the town—to whom he was married in 1871.

Wakefield was also associated with an event that later on was to bear fruit in a way that he little dreamt of, for it was in this town that he performed, on July 29, 1868, his first ovariotomy, he being in the first months of his twenty-fourth year. He performed this operation five times before he left Wakefield to settle in Birmingham in 1870.

That Tait should have attempted to perform ovariotomy is somewhat remarkable, for during his stay at Edinburgh he had had abundant opportunity to see what a fatal operation this always proved in the hands of others. Writing in 1898 he says: "As I have said in public more than once, I left Edinburgh with at least one conclusion determined in my mind, that I would never deliberately open the abdomen."

Now Tait in after-life showed that whatever his faults, lack of courage was not one of them; how, then, was it that he came to the determination that he would never open the abdomen? A sentence in his essay on "The Evolution of the

Aseptic Method" gives us the reason: "The results [of ovariotomy] I have seen in Edinburgh were truly awful, some thirty cases and not a recovery."

Now in order to understand why such operations as amputation of the thigh, ovariotomy, or even the simple radical operation for hernia, were attended with so great a mortality that an intrepid young man like Tait should determine never to venture on an abdominal section, we must glance at the conditions that existed in the great hospitals in the chief cities, not only of Scotland and England, but of the whole world at this period.

The truly awful conditions surrounding the surgical work of all large city hospitals had long been a subject that had vexed the kindly soul of Simpson, who as early as 1847 had been horrified to read in the report of the Edinburgh Infirmary, that out of eighteen cases of primary amputation performed during a period of four years only two of the subjects survived.

> He faced this fact with the courage of the reformer, and sought far and near for other facts to support the theory which he gradually evolved, that the melancholy failure of surgeons to save their patients' lives was due, not so much to the operation or the operator, as to the environment of the patient. In later years he himself often shrank, on account of unfortunate experiences, in performing capital operations, which he had formerly unhesitatingly undertaken.[1]

The result of Simpson's labours is contained in his essay on "Hospitalism," and almost his dying words were: "I would have liked to have completed 'Hospitalism,' but I hope some good man will take it up." The man who did take it up was his pupil Tait, as we shall see later on.

Let us glance at the condition of the larger hospitals at the period when Tait was a student in Edinburgh, for it is quite impossible to appreciate what Tait did in the early years of his career, until we have grasped the conditions under which surgical operations were carried out at this period.

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[1] "The Life of Sir James Simpson," Laing Gordon, 1897, p. 177.

This survey will also teach us two things: it will tell us why Tait at first became a follower of Lister, and why after a time he returned to the ways of his old master Syme, who was regarded as the safest surgeon of his time; [1] earning this reputation not only because he was a great operator, but because he carried out surgical asepsis, not from any knowledge of the real cause of sepsis, but rather because he was an aseptic surgeon by instinct, being always scrupulously clean and neat about his dress and person.

Let us hear what some well-known surgeons have to say about that pre-antiseptic period which embraces the years when Tait attended the medical school at Edinburgh from 1862 onwards.

Sir Hector Cameron thus paints the condition of surgery before the time of antiseptics:

> It is almost impossible to the young surgeon at the present day vividly to realise what the state of surgery was thirty-five or forty years ago.[2] The introduction of ether and chloroform some time previously had initiated great changes. Pain being abolished, surgery was robbed of no small part of its terror, and patients in their anxiety to be restored to health, submitted more willingly to such operations as were recommended for their cure. In this way, no doubt, a great impetus was given to the surgical art, new operations were devised and employed, while those which were old and well established were resorted to in a large number of cases. And so, uncertain as the healing of wounds had always been, the uncertainty and disappointment, which dogged the footsteps of the surgeon, became increasingly greater as the scope of surgery was extended. In almost all wounds, whether inflicted by the surgeon or by accident, inflammation and suppuration were expected as a matter of course. And these things were only the beginning of evil. No matter what skill and care had been expended on the making and dressing of the wound, the issue was necessarily looked forward to with doubt and anxiety. Few of those who did recover did so without serious episodes and complications of some sort. All suffered from pain and fever, more or less pronounced, and very many only reached the *terra firma* of restored health

[1] In 1896, Lister, when President of the British Association at the meeting held at Liverpool, spoke of Syme as the "safest surgeon of his time." Chiene, *Brit. Med. Jour.*, vol. ii. p. 1848, 1902.

[2] This was written by Cameron in 1902.

by swimming through weeks or months of severe and painful illness.

Secondary hæmorrhage, tetanus, erysipelas, septicæmia, pyæmia, and hospital gangrene were never all absent from the hospital wards, and at times pyæmia and hospital gangrene became alarmingly epidemic. In the absence of any certain knowledge of the real mode of causation of these wound-begotten diseases, and, therefore, of any sure means of avoiding their occurrence, the surgeon felt no real personal responsibility regarding them, whatever grief and disappointment he might experience when his best efforts repeatedly ended in disaster and failure. When his patients were decimated and his heart was well-nigh broken by these terrible visitants, he received the sympathy of his friends and pupils. He had done his work well, and a trail in harvest had come to destroy it. He was in no way to blame. He was a man beset by misfortune.

This was very much the state of matters in the Glasgow Royal Infirmary, at that time the only hospital in the city, when in 1861 Lord Lister (then Mr. Lister, a young man of thirty-four) was elected one of the surgeons.[1]

Tait himself has left us a picture of the condition of the hospitals when he was a student at Edinburgh. He says:

> Lister and I started on common ground. He entered the Edinburgh Infirmary as Assistant Surgeon in 1857, and I was a student in 1860. Syme was Professor of Clinical Surgery, and had a special building of his own, lighted and ventilated on both sides, two stories in height, and with a reasonable number of beds; the exact number I do not remember, but I should say about thirty. At least the rooms always bore a startling contrast in every way with those in the contiguous building in matters I shall refer to in detail, and yet the two establishments were only separated by two valves of the swing door.
>
> Personally, Syme was the personification of the best type of gentleman, always perfectly dressed in his old-fashioned way, and as clean as a new pin. From his boots to the top of his head, no one ever saw dirt, disorder, or the appearance of hurry. He was always washing his hands—I think I may say he washed them every time he touched a patient. His assistants had to be like him, and his old-fashioned nurses were noted for their tidiness and cleanliness everywhere.
>
> Through the folding doors, what a difference! Everywhere hurry and untidiness, and no mark of what I have

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[1] "Lord Lister and the Evolution of Modern Surgery," Sir Hector C. Cameron, *Brit. Med. Jour.*, December 13, 1902.

praised in Syme. There was a good deal of dandiness, but much dirt. I shall not venture on any detailed description, though all who took part in it as principals have gone to the majority, yet I might traverse into unintentional exaggeration, and I might hurt the feelings of some one who took part in the affairs of the old Edinburgh Infirmary after my time. Suffice to say it was a four-storied building, badly lighted, very badly ventilated, crowded with students for many hours of the day, with a very insufficient number of nurses, who were void of training, that the wards stunk, that they were overcrowded and marked by numerous faults in cleanliness and hygiene, which would not be tolerated now in the oldest fashioned institution in the country.

There is an authentic word picture of a similar state of matters at the same time, which will serve to show what Simpson meant by "Hospitalism." I shall quote it at length:

"The operating table was of wood and of fabulous age. It was sometimes thought necessary to have a new mattress for the table when the stuffing was found to be matted together in lumps by the blood, which had during many years soaked through its covering. The only correct garb of the surgeon was a frock-coat (the oldest and shabbiest in his wardrobe), which was kept in the surgeon's room and never renewed or cleaned during his twenty years of operating work."

Syme always turned up the sleeves of a dress-coat in which he might have shown himself, before the operation, before his Queen at Windsor.

The Operating Theatre attendant was permitted to employ his spare time in the post-mortem room, the surgeons came straight from the dissecting room to operate, after simply washing their hands; ligatures were used which had been already soiled by handling with blood-stained fingers to bind up wounds in a second case. And at Edinburgh these ligatures were always worn ostentatiously by the House Surgeon, like a badge of knighthood, in the button-hole of a coat, which often rivalled that of his chief for dirt and for condition. What were the causes of the extraordinary differences separated by the memorable swing doors? The prime and real causes were, first of all, that Mr. Syme was a master, that he held his appointment for life, he was a rich man, and he made his underlings do as he wished. Besides, he cared a great deal for results, as was shown in the notorious trials about perineal section. He was not merely desirous of doing an operation, but was desirous of seeing that patient cured and well years afterwards. Further, he was a great surgeon; in every sense the greatest I have ever seen. Nothing charmed him more than to show us a man upon

whom he had done some tremendous piece of work years before, and this alone can be the object of true surgery.

On the other side of the door, the men were on short tenure of office, and necessarily some of them quite incompetent for it. They dashed at their work, they hurried it, they crowded all they could into the ten years they had of it. I saw some awful things in my six years of pupilage in the Edinburgh Infirmary, things surgeons could not possibly bring about now if they tried.[1]

Those passages give us some idea of the condition of the hospitals in Tait's student days, and they show us that the result that was to be expected after even the most simple incision [2] was suppuration, and consequently, with this ever before his eyes, no young man wanted to attempt such a big operation as ovariotomy, which could only be expected to be followed by one result, and that a fatal one.

This emphasises what we have said in the opening lines of this chapter, that it required considerable courage on the part of Tait to tackle his first ovariotomy in Wakefield in 1868.

Naturally a young man is much influenced about an operation by the views held by his teachers. In this instance, however, Tait would be undecided, for it is a well-known fact that Simpson and Syme held very different views with regard to ovariotomy. Simpson maintained that ovariotomy was an operation worthy of serious consideration. He had pointed out that the operation had met with great opposition in all classes in the profession, and he had to confess that the statistics of the operation in Germany showed a very high mortality. He pointed out, however, that the results attained by Clay of Manchester and Atlee in America, and Spencer Wells in London, gave hope, in spite of a mortality that was over 30 per cent. In one of his lectures he said: "In my last lecture I endeavoured to prove to you that ovariotomy was a justifiable operation, and that though it was formidable in its character, and often fatal in its results, yet we are fairly entitled to place it in the rank of legitimate surgical operations." [3]

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[1] Lawson Tait, "The Evolution of the Aseptic Method in Surgery," *Med. Press and Circ.*, April 27, 1898.

[2] Velpeau had said of this period, "A pin prick is a door open to death."

[3] "The Works of Sir J. Y. Simpson," vol. iii. p. 497.

On the other hand, Syme would have given Tait little encouragement, for Tait says in one place: "Syme refused to perform the operation to the end of his life." [1] And in another place he says: "Had it not been for this unfortunate quarrel with Lizars in early life, Syme would never have been prejudiced against abdominal surgery, he would have established ovariotomy in the 'thirties,' and all that has since followed for it would have been done by him." Syme's views on ovariotomy about this time are given in his address on Surgery, delivered at the Annual Meeting of the British Association at Leamington.[2] He says:

> We now come to ovariotomy, which has of late been the subject of much attention; and it will here perhaps be supposed that a claim for the honour of priority may be advanced on the part of Edinburgh, where the operation was first performed. But to confess the truth, I fear that the Northern Metropolis, so far from deserving any credit on this account, should rather plead guilty in having invested the procedure with an aspect so repulsive as to impede, rather than promote, adoption."

Syme then goes on to refer in the most scathing terms to the cases reported by Lizars, and it was in connection with these cases that he had had his quarrel with Lizars, and as a result had a lifelong prejudice against abdominal surgery. Syme then continues his remarks, and says:

> These and similar cases so ludicrous, if they had not been so shocking, led surgical teachers to conclude that the operation laboured under three serious objections: (1) the uncertainty of prognosis; (2) the difficulty of diagnosis; and (3) the danger of execution; thence it happened that notwithstanding more favourable reports that after a time reached us from Manchester, with the exception of a few cases, all of which proved fatal, no further attempt was made in Scotland to establish the procedure until a recent period, when the successful experience of some gentlemen in London, and more especially Mr. Spencer Wells, gave the matter an entirely new position.

Knowing these to be the views of his great teacher, we do not wonder that Tait left Edinburgh, as he tells us in

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[1] Tait, "Diseases of the Ovaries," 4th ed. p. 257.
[2] The *Lancet*, August 12, 1865.

one place, with a firm resolve not to " deliberately open the abdomen." " But, in 1868, I broke through my firm resolution, and of my first ten ovariotomies only one died."

We have been unable to find any detailed account of these operations, but I have ascertained that his first case was the one that died. This fact is recorded by Tait himself in a short note under " Statistics of Ovariotomy " in the *British Medical Journal,* June 1, 1872. There he says: " The following represent the whole of my experience (of ovariotomy). All the cases occurred in private practice." Tait then gives a list of nine cases, and of these the first was " aged twenty-six, married, died of peritonitis." This operation was performed on July 29, 1868.

This does not, however, appear to have been his first abdominal section, because in his work on " Diseases of Women " (1877, p. 292) he says:

> Sometimes we open the abdomen to remove the ovarian tumour, and find that we are unable to complete the operation. This happened to me early in my experience of ovariotomy, and the case was an instructive one in many respects.
>
> In May, 1868, I saw Mrs. Aitch in consultation with Mr. Kemp of Castleford. She was thirty-seven years of age.

Tait then proceeds to say, " that on examining this patient he felt through the parieties a floating tumour," and when he operated he made the usual incision and " found that the cyst was a unilocular parovarium cyst, surrounded by a mass of soft cancer." He was unable to remove the cyst, so he closed the wound. The patient recovered but lived only seven weeks.

Tait evidently did not regard this as ovariotomy, and as he did not remove the cyst, he did not record this case in the above-mentioned list.

About his life in Wakefield, we have next to nothing to relate. His wife told us that he performed several operations that were unusual, and these attracted the attention of the townspeople who held him rather in awe, as the report was circulated that he had operated on a woman's heart; had in fact taken it out, repaired it, and put it back!

We may here note that he began to write letters and communications for the medical journals when at Wakefield, and this became a passion with him for the rest of his life.

While Tait had been a student at Edinburgh, he must have had a fair experience of the warfare [1] carried on in the medical papers by the leaders of the profession, and the past-masters of this form of recrimination were Syme and Simpson.

Tait, no doubt—if he was indeed Simpson's son—inherited from his father the ready gift of writing, and he certainly possessed the faculty of attacking other men's views in a very pertinacious manner.

While at Wakefield he began to write, and the first paper that I can find mention of was contributed to *Science Gossip*.[2]

Tait states in this paper that in August, 1867, happening to be walking by a spot where swallows congregated, he found one of the little birds, which had just died. It was still warm. On dissecting the swallow, the pleural surface of the lungs was studded with minute yellowish-grey points, which presented under the microscope all the character of tubercle. Similar nodules were found in the mesentery, which, he says, were glands probably in a state of tuberculisation.

In the *Lancet* of January 18, 1868, Surgeon Gentles notices Tait's communication, and draws attention to "another disease which seems to affect the feather tribe," *i.e.* tapeworm. Tait replies to this letter,[3] and gives his experience of tapeworm in birds. He states he has seen it in rooks and grouse, and he suggests that it is due to birds having partaken of dog droppings.

This letter elicited a reply from a writer signing himself "Pediculus," [4] in which he expressed surprise at Tait's views, and says: "Now Mr. Tait, in his letter to you, boldly suggests birds getting tapeworm by swallowing voa or a pregnant proglottis, thus throwing a doubt on the results of modern

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[1] "It must be remembered that the medical polemics of the Scotch capital was for a long time in the worst possible condition."—"Autobiography of Samuel D. Gross," vol. i. p. 290.

[2] Note in the *Lancet*, January 4, 1868.

[3] The *Lancet*, January 25, 1868.

[4] The *Lancet*, February 1, 1868.

research, and carrying us back to the time when neither the hydatid nor the tænia were at all understood."

Tait replies to this [1] in the following manner :

> As Spencer Cobbold says that, contrary to general expectation, the cestoid entozoa appear to be quite as abundant in the granivorous birds as they are in the carnivorous, and as it seems to me impossible to understand this on any other supposition than that the strobila may in them be developed from the ovum, or at least that the changes from the ovum to the cysticercus, and thence to the zooid, may take place in the one animal. I shall retain this as my opinion until "Pediculus," or any other authority, may give me good reason to alter it.

During the year 1869, we find some letters written to the *Lancet* [2] on the subject of acu-pressure. A sentence here and there is of some interest. For instance, he says : "Some years ago, I undertook, at the request of Sir James Simpson, a series of experiments to determine the process of union in the walls of vessels after the application of acu-pressure. Since then, as occasion offered, I have repeated these experiments." Again he says : "I am quite certain that no experiments on the vessels of the lower animals will be satisfactory unless these results are confirmed by clinical experience." He ends the letter by saying : "Professor Ogston will be satisfied of this if he waits until the appearance of the second part of Professor Spence's 'Text Book of Surgery,' in which, I believe, that distinguished surgeon intends to insert a coloured plate of a brachial artery which I acupressed most effectually without the division of coats, and which was permanently occluded, and the clot adherent, in thirty-eight hours."

This letter is dated March 20, and was written from Wakefield. On June 5, in the *Lancet*, he replies to a critic who suggests "that Mr. Tait should publish his experiments," by referring to the *Medical Times and Gazette* for 1865 and 1866, "where he will find some of my experiments detailed, and my conclusions derived from them given at length."

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[1] The *Lancet*, February 8, 1868.
[2] The *Lancet*, May 15, 1869.

Later in this year we find a short letter in the *Lancet* on the treatment of Intermaxillary Cleft. He refers to Mr. (afterwards Sir Thomas) Smith's operation for cure of cleft palate; chloroform being used during the operation. Tait says:

> Would you allow me to say in reference to Mr. Thomas Smith's paper, that I have now operated on many cases of cleft palate with chloroform, and without any other gag than a silver spoon; that my success has been greater than Mr. Smith's, if I am to judge from his own account, and that in this operation, as in all others, I think special and complicated mechanisms, however ingenious, ought to be avoided if possible.[1]

Later on, in September,[2] he has a short paper on "A New Method of Using Needles for the Operation of Hare Lip." He proposes

> to introduce two ordinary seamstresses' needles, armed with a few inches of silver wire doubled through the flaps, in the form of a St. Andrew's Cross; the point of which needle is to be introduced through the mucous membrane of the lip, about half an inch from the edge of the flap, and brought out of the middle of the incision, then introduced into the other flap at the point opposite, and brought out at the root of the ala of the nose. The needles cross in the middle of the wound. The flaps are to be carefully adjusted, then the heads of the needles to be pushed fairly into the lip, and pulled together by twisting the wires; the point of the needles are then to be cut off close to the section, and the stumps retracted into the flaps. In this way nothing is left to "catch," and when the needles are removed by untwisting the wires and pulling them, there are no scars left.

In February of the following year, he wrote a short paper to the *Lancet*,[3] "On the Use of Simple Syrup as a Collyrium." Later in the same year he has a paper,[4] one column in length, on "Case of Complicated Lithotomy successfully treated." He relates that he encountered a very enlarged prostate, which he almost divided in entering the bladder, and after

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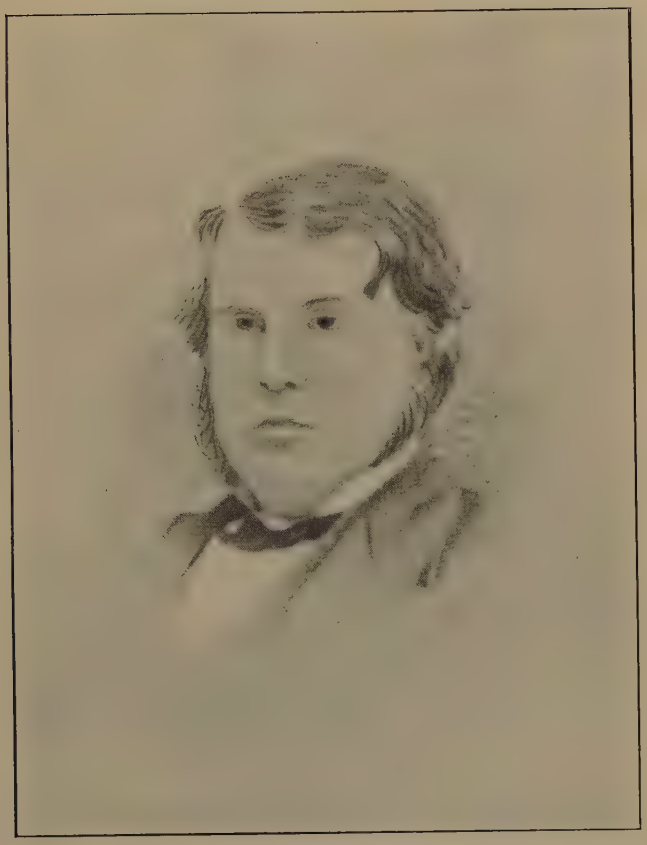
[1] The *Lancet*, August 21, 1869.
[2] The *Lancet*, September 25, 1869.
[3] The *Lancet*, February 12, 1870.
[4] The *Lancet*, April 2, 1870.

PLATE VI.



LAWSON TAIT. 1869.

To face page 22.

removing a calculus weighing five hundred grains, he continues: "after its [calculus] removal, examination of the wound showed me the segments of the tumour had been partially dislodged; that it was capsulated, and might be removed. I was strongly inclined to attempt its removal, but having waited a few minutes and found that there was no bleeding, I hesitated to run further risk." He goes on to say that he wrote to Sir William Fergusson about the case, and that surgeon thought that he would have removed the tumour.

On May 14 we find him writing a letter to the *Lancet* [1] on "Operation of Extirpation of the Coccyx." This letter was written because Dr. Nott in the *American Journal of Obstetrics and Diseases of Women and Children*, had claimed that he was the first to extirpate the bones of the coccyx for the cure of coccygodynia. Tait says: "It is more than eight years since I assisted Sir James Simpson to extirpate the bones of the coccyx in a case of inveterate coccygodynia, for which repeated subcutaneous incisions had been made."

In October he wrote a paper of a column and a half to the *Lancet*, giving the results of his treatment of tetanus by wourali, Calabar bean, and chloral hydrate. All the cases ended fatally.

His paper on tetanus was the last he wrote at Wakefield, as he left that town for Birmingham about this period.

In reading through these contributions to the *Lancet*, one notices that Tait, thus early, did not place any great reliance on the result of experiments on the lower animals, and that he was early a believer in simple instruments. These two beliefs he never got away from, and most of the instruments that he invented in later life were remarkable for their simplicity, while his views on vivisection have become only too well known.

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[1] The *Lancet*, May 14, 1870.

III

Birmingham

1870

Tait came to Birmingham in 1870.

A man with such a restless temperament could hardly be expected to remain in a quiet little town such as Wakefield was in those days, so he looked out for a field where his surgical powers might find a wider scope ; he chose Birmingham.

Once we asked him " Why ? " and he said : " Because there was no pupil of Simpson's there, and it was the centre of England, and if a man became well known, he could be called in more quickly in consultation than if he lived in London."

In Birmingham he acquired the practice of Dr. Thomas Partridge, who was anxious to leave Birmingham for Gloucester, and had offered his practice for sale. To him, at his house at the corner of Burbury Street and Lozells Road, came Tait from Wakefield. The practice was a family one in a populous suburb, and after a short introduction was left in Tait's sole charge.

The taking over of Dr. Partridge's practice was for Tait only a means to an end. Before coming to Birmingham he had armed himself with many letters of introduction to members of his own profession, and to a few prominent citizens ; he knew that his first move should be to make himself known, not only to his professional brethren, but to others who might help him on.

He began what proved to be a life's campaign at the local medical societies, for joining in all the debates he contraverted the opinions of his seniors, quoted his own experience

in opposition to the juniors, and soon became known to every member of the profession in Birmingham.

Tait was poor, he had little or no money, and while the Lozells practice brought in some return, he soon resorted to a method of augmenting his funds which not only proved financially successful, but served to bring him in touch with public men, who quickly recognised his ability and aided him in those first months when help is always doubly useful to a stranger in a strange land.

Among the men whose acquaintance he made at this time was George Dawson, the editor of the *Morning Post*. Dawson, at this time, was a man who dominated the public life of Birmingham. His influence was tremendous. He was not only a brilliant preacher, but he was a lecturer, a scholar of wide learning, and a journalist of the first rank.

When Dawson had first arrived in Birmingham he had accepted an invitation to preach on trial at a Baptist Chapel, during the latter months of 1844. He preached for twelve months, but some differences, doctrinal and legal, led to his resignation, and so his followers built him a new chapel, which was opened in 1847, and there, until his death in 1876, he attracted an immense following.

"Humanity" in its broadest sense was the keynote of his life and work, both on the platform and in the Press. History and politics were the very breath of life to Dawson, and no public question arose in Birmingham that was allowed to pass without his skilled criticism.

It was not in Birmingham alone that he was respected. The people of Manchester and many other towns in England held him in great respect, and for thirty years he lectured all over England.[1]

"He was the companion of Carlyle on his first visit to Germany, and he walked by the barricades of Paris with Emerson in 1848."[2]

[1] Lord Morley, in his "Recollections," tells us of the people that he met at Chamberlain's house at Birmingham, and among them was Dawson. He says: "A fourth was George Dawson, the friend of Mazzini, Carlyle, Emerson, who has done as much as any Englishman of his time by the picturesque force of his lectures, both in Birmingham and other large towns all over England, to awaken curiosity and popular interest in the great names of literature and history."

[2] "Dictionary of National Biography," vol. xiv., London, 1888.

When Dawson met Tait he quickly sized up the young surgeon, and suggested that he should join the staff of the *Morning Post.*

Tait was only too willing to accept an opening which enabled him to augment his funds, and brought him into touch with the public life of a large town; after a short period he began to write the leading articles for the *Post.*

Tait and George Dawson became firm friends; he always spoke of Dawson in the most affectionate terms, and readily acknowledged the great debt of gratitude he owed to one who had so powerfully influenced for good his intellectual development.[1]

Tait had not settled long in Birmingham before he was forced to write to the *Lancet* and acknowledge that he was in error on a subject upon which he probably thought that the claims of his teacher, Simpson, were beyond question.

The matter arose thus: In the *Lancet* of May 14, 1870, he wrote a letter which we have noticed in the preceding chapter, on "The Operation of Extirpation of the Coccyx." The letter was written because Dr. Nott, in the *American Journal of Obstetrics,* had claimed that he (Nott) was the first to extirpate the bones of the coccyx for the cure of coccygodynia. Tait claimed that Simpson was the originator of the operation, and that he had helped him do the operation eight years previously.

Dr. Nott, in a letter to the *Lancet,*[2] in reply to Tait, said:

> In the May number of the *New Orleans Medical Journal,* 1844, twenty-six years ago, was published my first case. About a year after, I published a second case. The two specimens were presented to Dr. Miggs of Philadelphia, who alluded to my cases every year afterwards, till he resigned his chair, in his lectures in the Jefferson Medical College.

Nott's claim being now beyond dispute, there was no other course left for Tait but to write to the *Lancet,*[3] and

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[1] There are two statues of Dawson in Birmingham, one in the open, and the other in the entrance hall of the Free Library.

[2] November 5, 1870.

[3] November 12, 1870, p. 654. This letter is signed from Waterloo Street, Birmingham.

say: "Under the circumstances narrated in his letter, I am quite prepared to admit Dr. Nott's claim."

As he had to admit that his master had been forestalled in the matter of the extirpating of the coccyx, he soon put in another claim for him, and accordingly we find him a few weeks later contributing a paper, "On the Cure of Chronic Perforating Ulcer of the Bladder by the formation of an Artificial Vesico-Vaginal Fistula." [1]

Tait began his paper by pointing out that, while Simpson gave much to the profession, "it is to be feared that there died with him a great deal that would have been of infinite value to humanity had it been saved to us." Tait, therefore, proposed to give some of the many observations made, but not published. "It is with a view to saving a little of these, that I venture to refer to two cases in the treatment of which I was associated with him, and in which the successful operation employed was a device original with him, and, as far as I have yet been able to discover, quite new in the annals of surgery."

Tait then related the cases and the treatment. In the narrative about the first case of a young married woman, he says:

> She at last was placed under the care of Sir James Simpson, and as we came one day together from her bedside, he reasoned to the effect that the indication in her case was to put the bladder in a state of complete physiological rest; and to secure this, it would be necessary to establish an artificial fistula. He carried his idea into execution, and with the most brilliant result, for the pain was at once removed, and in a few weeks the ulcer was healed, the fistula was closed, and the woman returned to her country house perfectly recovered.

Tait, by a contribution such as the above, had more than one purpose in view: ostensibly his object was purely philanthropic, for in the concluding lines of his paper he says: "If by making these two cases of Sir James public, I shall at any time be able to place in the hands of my brethren a successful means of treating a terrible disease, my object will be accomplished."

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[1] The *Lancet*, November 28, 1870.

We will give him credit for the best motives, but we cannot forget that in every paper that he wrote he never failed to bring in, if possible, his connection with some distinguished man in the profession. So here he does not lose his opportunity, " and as we came one day together from the bedside he reasoned." This was really one method that he adopted to impress his Birmingham confrères, and soon after this he tried again to impress them when he was able to publish the concluding propositions left by Simpson on " Hospitalism," the papers having been handed over to him by Simpson's son. Tait accordingly took the opportunity to write a short introduction to the papers, in which he states that " to the further elucidation of this question, I mean to devote myself; and I will earnestly beg the readers of the *Lancet* to aid me as far as they can, should I have to appeal to any of them for statistics." [1]

Towards the latter part of this year, 1870, Tait determined no longer to continue as a general practitioner. Once having made up his mind, he did not hesitate to announce to the world that he wished in future to be considered a surgeon, and to prove his quality, he passed the examination for the Fellowship of the Royal College of Surgeons, Edinburgh; he then abandoned the general practice he had taken from Dr. Partridge, for to such a man as Tait this family practice at Lozells was too small a thing, and he had not the patience to make such a humdrum existence a success. His next step was to associate himself with Dr. Bell Fletcher [2]—a well-established practitioner; and in the early part of 1871 we find Tait announcing himself as a Consulting Surgeon.[3]

To set up as a consulting surgeon [4] at the age of twenty-six required much courage. Courage, however, as Reed remarks, was a quality in which Tait was never wanting.

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[1] The *Lancet*, November 19, 1870.

[2] He was senior physician to the Birmingham General Hospital.

[3] His letters to medical papers after this period are signed from Waterloo Street, Birmingham.

[4] " No one in Glasgow pretended at that time [1860] that his practice was purely surgical. Indeed, the idea of a really pure consulting practice of any kind was hardly grasped, even in London, until many years later." —" Lord Lister," p. 87, Godlee.

His self-assertion commanded recognition. The community began to estimate him as he estimated himself. The consulting surgeon gradually became the specialist.[1]

His next step was to endeavour to establish a Hospital for Women, and as he hoped to become known as a gynæcologist, the first move on his part was to make himself known to some of the women in Birmingham. Accordingly the plan that he adopted was to obtain the appointment of " Lecturer on Physiology " to the Ladies' Class at the Midland Institute, an important educational centre in Birmingham. He received the appointment through the influence of Dr. Heslop, and he lectured on physiology at the Institute from 1871 to 1879.

When at Birmingham, in 1891, we were spending a week end with Tait and his wife at his country house, and Mrs. Tait reminded him of the ladies' class, Tait with a smile said: " That class was really the means of giving me my first important lift in life, for all the members helped me." Tait knew that *Fœminæ sunt medicorum tubæ*, and he used the " war trumpets " to proclaim that he existed.[2]

In the opening months of 1871, Tait could have had little spare time. He had to attend to his practice, and to assist Dr. Bell Fletcher; he had to read for his English Fellowship, and to work up enthusiasm and find supporters to carry out his scheme for a new hospital, and contribute a leader to a daily newspaper. Yet with all this, he did not fail to keep himself before his colleagues' notice. Accordingly, we find him early in this year tilting at the " Antiseptic Treatment." He contributed a paper on " Cases treated antiseptically on Lister's Method," [3] and he concluded the paper thus:

> In the treatment of abscesses by the carbolic method, I have been invariably unsuccessful, despite the most persevering

[1] In an oration delivered before the Huntarian Society in 1861, Sir William Gull took occasion to utter a protest against the popular prejudice for specialists. Specialism was not a new idea. If we are to believe the account given by Herodotus, specialism originated in Egypt.

[2] Osler says: " Women, our greatest friends and our greatest enemies, are the chief sinners, and while one will exhaust the resources of the language in describing our mistakes and weaknesses, another will laud her pet doctor so indiscriminately that all others come under a sort of oblique condemnation."

[3] The *Lancet*, January 14, 1871.

> and honest trials of Professor Lister's various methods of dressing, and all the precautions recommended by him. My opinion of the antiseptic treatment is that its merits have been greatly overrated, and its good results, which are quite as uncertain as those of other means, are due more to the great care taken of the cases than to the exclusion of the air.

This, then, must have been his first hostile note against Lister's methods, and his hostility was fated to grow and increase as the years passed, until it became impossible for him to do justice to the question.

" Hospitalism " gave him an opportunity to air his views. Having, as mentioned above, been entrusted with the final portion of Sir James Simpson's work on " Hospitalism," and having published this in the *Lancet*, he now undertook to defend Simpson's views against Mr. Holmes, the well-known London surgeon. He turned the subject also to his use to aid those who were advocating the advantages of small special hospitals. He says :

> Does Mr. Holmes know the statistics of ovariotomy ? If not I can tell him about them, for I have collected all that I believe are to be had, having had the kind assistance of Mr. Spencer Wells in the matter, and I find that while in small hospitals in private practice, the mortality of this serious operation does not reach 30 per cent., in large hospitals it is more than 60 per cent.

He adds in a note :

> It is only fair to say that Dr. Keith tells me he does not attribute his brilliant successes to his small hospital. He is about to try the experiment of a room in the Edinburgh Infirmary, and I trust that the experiment may be more successful in his hands, than it was with those who tried it about eight years ago." [1]

But he was not content with this single paper, for in a few months' time he returned to the attack, and he contributed a paper to the *Lancet* entitled, " A Consideration of the Criticisms advanced on Sir James Y. Simpson's Papers on ' Hospitalism,' especially those of Mr. Holmes." [2]

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[1] The *Lancet*, January 28, 1871.
[2] The *Lancet*, April 1, 1871.

He opened this communication in the following words :

> The position I have taken up in this controversy is one confessedly of difficulty and danger ; for, on the one hand, I may lay myself open to the charge of following blindly the views of one to whom I, like many others, owe very much ; and on the other hand, I may be blamed for presumption in criticising the opinions of those above me in seniority, as in all else, in my profession. I shall, however, avoid the weakness of being *addictus jurare in verba magistri ;* and if I have to offer criticisms adverse to the opinions of men like Mr. Holmes, Mr. Callander, and Dr. Mathews Duncan, I trust that they will take them as honestly intended, and arising from a study of the subject as full as I can make it.

When he deals with Mathews Duncan's book he gives us a glimpse of that nice method of criticising others' views, which made him so many bitter enemies in later years. He says :

> This is remarkably the case with Dr. Mathews Duncan, through whose elaborate book I have gone carefully without finding anything bearing on any points other than the treacherous memories and the " amiability " of the gentlemen who were asked for returns. When we bethink ourselves that for the " amiability " a much uglier word might be substituted as a tolerable literal rendering. . . ."

Although there is a notice at the end of this paper that it is to be continued, we cannot find any other communication on this subject. Tait afterwards returned to the subject in his book on " Hospital Mortality," published in 1877.

Early in 1871, reports began to come from Birmingham that a scheme was on foot for establishing a special hospital for the diseases of women. The *Lancet* [1] heard of the scheme, and thought it necessary to give a timely warning :

> Scientific and administrative experience is against the multiplication of special hospitals, and in favour of developing special departments in general hospitals. Science is rarely promoted, the profession frequently injured, and much public money wasted by endeavours to enclose fields for practice for enterprising specialists. We do not deny exceptions, while deeming it our duty to utter a word of caution.

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[1] The *Lancet*, February 11, 1871.

But Tait and those who were helping him were not to be turned aside and worked with a will. They found that the chief objectors were those who maintained that the greatest possible amount of good could be done with any money subscribed if it were devoted to the formation of a special department in one of the large general hospitals.

Tait, however, had already given much attention to hospital mortality, and he took up the position that, as the results of such a typical abdominal operation as ovariotomy were so bad when the operation had been performed in connection with large general hospitals, the performance of such an operation, in any but a small special hospital, was an utterly unjustifiable procedure.[1]

A public meeting was held at Birmingham, March 3, 1871, and it was resolved to start a hospital to be devoted to the diseases of women. A committee was formed to carry out the project, and they rented a house, No. 8, "The Crescent." They first decided on the consulting staff, and Dr. Fleming and Dr. Bell Fletcher (Tait's partner) were appointed consulting physicians. The Managing Committee consisted of eighteen; nine of their number being women. The real work of the hospital was to be done by four surgeons, who had to be elected by a special committee appointed for the purpose. The qualification for the office of surgeon was fixed at the Fellowship of the London College. This was awkward for Tait, as he had only a year or so before become a member of the London College; but he threw himself into the work, and in a remarkably short time acquired the requisite diploma.[2] The late Mr. T. H. Hartlett, who was closely associated with him at the time, was lavish in his praises as to the ease and rapidity with which Tait prepared himself for this examination. It was just the kind of lesson the profession in Birmingham required at the time to show them that the new-comer was a man of real knowledge and ability.[3]

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[1] In order to express the terror inspired by ovariotomy, a physician went so far as to say that it should be "classed among the attributes of the executioner."—René Vallery-Radot, "Life of Pasteur," vol. ii. p. 16.

[2] Admitted June 8, 1871.

[3] *Brit. Med. Jour.*, June 24, 1899.

In July, the election took place. The candidates were ten in number, " and embraced the cream of the rising members of the profession who are unconnected with the present hospitals. Mr. Bracy, M.D., surgeon to the Children's Hospital, Dr. T. Savage, F.R.C.S., and Mr. Lawson Tait, F.R.C.S., were the successful candidates." [1]

Tait, and his colleague, Savage, made this hospital famous. They were connected with it for many years, and it was not until 1893 that Tait resigned. In this Institution was carried out much of the original work that has made his name famous in every country where abdominal surgery is practised.

The only other contribution to the *Lancet* that he made at this time was a letter on the " Strange Course of a Uterine Sound." [2]

Hildebrandt had some time before published two cases where a sound, introduced into the uterus, could be felt at the umbilicus. Hoening, of Bonn, published an analogous case. These authors differed as to the supposed cause of this curious travelling of the uterine sound; Hildebrandt thinking that the instrument had passed through one of the Fallopian tubes, and Hoening that the sound pushed upwards the walls of the atrophied uterus.

Mathews Duncan contributed a letter on the subject, and referred to a paper which he had written in 1856 on the unnatural potency of the inner end of the Fallopian tube. Tait's letter says:

> In your foreign gleanings of April 15, you refer to the cases published by Hildebrandt and Hoening, in which the sound perforated the fundus of the uterus, and you suggest that it would be interesting to know if such a phenomenon had been observed in this country. It is quite well known, and was especially so to the late Sir James Simpson, who regarded it as of no consequence. I remember being present when a gentleman exclaimed to Sir James that he could not reach the fundus. " Oh, you have gone past it," was his answer. It has happened twice to me to perforate the fundus

[1] The *Lancet*, July 15, 1871.
[2] The *Lancet*, April 29, 1871.

unwittingly, and I intentionally made use of it for the purpose of diagnosis on one occasion.

In 1889, when Tait published his work on " Diseases of Women and Abdominal Surgery," he referred to the subject under the heading of Metro-Peritoneal Fistula in the following words :

> Years ago, Simpson drew attention to the fact that occasionally when the sound is used in gynæcology, it will pass through the fundus into the peritoneal cavity, without in any way injuring the patient. A great deal of scepticism has been expressed about Simpson's assertion—in private of course—and it was not until I made a series of clinical displays of the fact that it was generally admitted. An eminent German surgeon openly disputed the possibility of its occurrence, but since has had reason to admit its actual occurrence. Certainly nothing is likely to startle one more than to find a sound pass five, six, or seven inches inwards, when there was no reason to suspect that it would penetrate more than two and a half. I had often heard Simpson speak of it, but the first time I saw it, the event had an almost theatrical absurdity. An eminent continental Professor of Midwifery was visiting Simpson, and we were showing him cases in Simpson's enormous private clientèle. He was using the sound to replace a retroflexed uterus, when he suddenly found it went inwards, and in gently pushing it up, it entered to the hilt. He was greatly distressed under the belief that the sound must have entered a pregnant uterus, and when Simpson told him that that was impossible, and that he had perforated the abdominal cavity, his distress was most painful. We could only assure him that there was no danger by exhibiting to him the patient perfectly well, three or four days afterwards. An explanation of such cases, to the effect that the sound travels along a Fallopian tube, which has been offered by Dr. Mathews Duncan, would, if correct, be really a more wonderful thing, than the more simple one of perforation of the fundus. When I first heard of Dr. Duncan's theoretical view of the permeability of the Fallopian tube some twenty years ago, I regarded it as very unlikely, and I suspected that it was based upon the fact, that when Simpson said " white," Mathews Duncan uniformly was of a " black " opinion. There was no doubt the fact that the sound did travel into the peritoneum somehow. Simpson thought that it went through the fundus, and therefore Duncan must have some other explanation. Not only would the sound have a difficulty in finding the internal aperture of the tube, but there is a still greater unlikelihood of the tube being placed in the

axis of the uterus, and being at the same time so freely movable as to allow the perforating sound to pass in all directions within the abdomen.

Tait then quoted cases to show that the explanation of the ready passage of the sound in some cases is to be found in the presence of a metro-peritoneal fistula.

IV

The First Diseased Ovary Removed

1872

The year 1872 had not advanced far before Tait performed an operation that has now become historical. He has left us the following account of the case and the operation.[1]

> In October, 1871, Mr. Hallwright, of Summer Hill, Birmingham, asked me to see with him a woman, aged forty-two, who had suffered for years from agonising pain in the pelvis, chiefly on the left side, distinctly referable to the seat of the ovary, accompanied by a variety of reflex symptoms, of which the most marked was complete and persistent aphonia. She had for many years been under treatment at the hand of Mr. Hallwright and others, without the slightest relief. We found the left ovary large and tender, displaced down behind the uterus, and the slightest pressure upon it gave rise to the characteristic sickening pain. From this pain she suffered in defecation. After having seen the patient many times, and after careful thought, I ventured to suggest to my colleague the removal of the ovary would probably cure her. I recognised the gravity of the proposal, for I had no fear that her suffering would kill her, and she had only some six or eight years to live before the climacteric period would bring relief; but both Mr. Hallwright and I thought it was better that suffering should be saved her for that period, even at some risk of life, than that it should be continued, making life a burden to her and all her surroundings. The patient and her friends arrived at quite the same conclusions when the matter was explained to them, and the operation was decided upon. With the assistance of Mr. Hallwright and Mr. Bennett May, I performed it on February 11, 1872, and found the ovary non-adherent, as large as a pigeon's egg, and full of thick, grumous matter, which, at the time, I took to be the fat of a demoid cyst. More careful examination has since made me believe that it was a chronic abscess, for there is a complete absence of any skin structure in the walls, and there were no appearances of any of the hair, teeth, etc., which characterise

[1] "The Pathology and Treatment of Diseases of the Ovaries," Lawson Tait, 1883, p. 323.

dermoid tumours. So far as I know, this is the first record in the history of surgery of the removal of a small ovary on account of pain. The patient made a speedy and complete recovery, and has since remained completely free from pain in the pelvis. Her voice returned and is now of normal power. She suffers now from some obscure disease which has stiffened her knee-joints and makes her a cripple, but all the symptoms which were in existence before the ovariotomy were completely and permanently cured by it.

In the above passage, Tait claimed to have been the first surgeon who deliberately operated to remove an ovary because it was causing pain. This being the case, it will now be necessary to go into the history of the operation of the removal of the ovaries to see if his claim can be made good. We shall, therefore, glance at the history of the operation from the earliest times down to 1872. We shall not here bring in instances of the removal of ovarian cysts, which will later on be dealt with when we come to consider the operation of ovariotomy; here we shall show that ovaries of normal size, or only a little enlarged, were removed before the date of Tait's operation, February, 1872; but we hope to prove that Lawson Tait was the first surgeon who did a deliberate and set operation to remove a small *diseased* ovary, because that ovary was diagnosed as the cause of the patient's suffering.

Among the ancient classical writers, castration of male animals is mentioned by Aristotle, Varro, Columella, and by Palladius. In the case of female domestic animals such as cows, camels, sows, bitches, mares, and ewes, the writings of Pliny, Galen, and Suidas make it clear that castration was frequently practised. Celsus and Paulus Ægineta have left us descriptions of the operation of castration when performed on man, and Juvenal tells us that the surgeons of his day were often called upon to perform castration; while Abulphargius has recorded that the operation was very frequently performed in Baghdad.

As this operation was so commonly carried out on men, and on male and female domestic animals, it is not surprising that we find it reputed that the same operation was performed on women. Athenæus, called by Suidas, γραμματικός or

literary man, who wrote during the second century A.D., has left us a work entitled the "Deipnosophistæ," *i.e.* the "Banquet of the Learned." It is a collection of anecdotes and extracts from the writings of poets, historians and physicians, of facts of natural history, criticisms, and discussions on almost every conceivable subject. In the twelfth book, chapter xi., he makes the following statements: The Lydians went to such a pitch of luxury that they were the first to castrate women;[1] Xanthus, in the second book of his affairs of Lydia, says that Adrainyttes, the King of the Lydians, was the first man who ever castrated women, and used female eunuchs instead of male eunuchs.

Later on Gyges, who ascended the Lydian throne about 700 B.C., is reported to have removed the ovaries from women with a view to prolonging their charms and bestow upon them perpetual youth.

Authors in the past, however, do not agree as to the precise character of the operation, for while Strabo and Diemerbroeck assert that the ovaries were actually extirpated, Adolphus states that the uterus was removed; and others still, that only circumcision (probably clitoridectomy)[2] was performed (Peaslee).

The late Greig Smith was of opinion that if the Lydians really castrated animals for domestic purposes—and of this there can scarcely be a doubt—it is quite within the bounds of possibility that they castrated women also.[3]

Our own opinion is that among Eastern nations in ancient times the ovaries of women were probably removed, in some cases for æsthetic reasons, in other cases for religious motives, while in most instances it was done merely to have in attendance at the palace of some despot, female eunuchs instead of male eunuchs, to act as chamberlains or guardians of the bed.[4] Judging by the picture that Athenæus

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[1] ὡς καὶ πρῶτοι γυναῖκας εὐνοῦχισαι.

[2] That the clitoris was frequently removed is quite clear from the description left us by Paulus, and other writers. "Paulus Ægineta," Book VI., sec. lxx.

[3] "Abdominal Surgery," 5th ed., vol. i. p. 236.

[4] The term used by Athenæus is εὐνοῦχισαι, from εὐνουχίζω to make a eunuch of: εὐνοῦχος, a eunuch, is connected with εὐνή, a bed, hence eunuchs were the guardians of the bed.

has left us of the vicious habits of the Lydians, they seem to have been quite capable of any extreme measure in connection with females. That the operation was ever performed for therapeutic reasons need not be considered for a moment.

Coming now to modern times, we have the statement of Jean Wierus, who, in his work "De Præstigiis," [1] has recorded that an Hungarian sow-gelder, being disgusted by the lewdness of his daughter, exercised his skill on her, and removed her ovaries.[2]

Martin Schurig, who wrote on Obstetrics (1733), speaks of two cases similar to that related by Wierus. Plater [3] of Bâle has left us the observation that he considers it quite a possible operation to remove the ovaries from any woman who shows an excessive tendency to sexual passion.

In quite recent times it is stated that castration was practised on females in Central Asia. Dr. Roberts, a medical missionary, is said to have met, in Bombay, Hedgeras—attendants in harems—who were spayed. He remarks that they did not menstruate, and that they had no sexual desire.[4]

There existed in a portion of Russia, and also in Roumania, a religious sect termed the Scoptz,[5] the members of which are said to have practised as a religious rite the castration

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[1] "De dæmonum præstigiis et incantationibus," Bâle, 1564.

[2] Those who wish to trace the operation back to remote periods will find much to interest them in the little treatise, "De Castratione Mulierum," by Georgius Francus, 1673; the chapter "De Castrandis Mulieribus," in a book by Père Theophile Raynaud, 1655; Dujardin, "Hist. de la Chirurgie," Paris, 1774; Mahan, "Médicine Légale et Police Médicale," Paris, 1801; Pierre Dujour, "Historie de la prostitution chez tous les peuples du monde," Boinet in the article "Ovariotomie" in the "Dict. Encycl. des Sc. Med.;" George Frank de Frankenau in the first section of the second "Satyra" gives the pith of what was known upon the subject up to his time of writing (1722), and he also gives a good bibliography.

[3] "Observationum, libri tres," Bâle, 1614.

[4] "Hedgeras de l'Asie Centrale," *Journal l'Experience*, 1843.

[5] "The Skopsi of Russia began about 1757 A.D.; they emasculated themselves and practised flagellation. The order thrived in spite of much persecution from the authorities. In 1815 the prioress of a flagellant society introduced the practice among women, and girls and young women allowed themselves to be spayed (ovaries cut out) and to have their breasts cut off, so as to be less able to excite sexual desire in the men. The sect thrived, and while it is not accurately known how many have been mutilated, it has been stated that the sect numbers about 150,000 members." Wall, "Sex and Sex Worship," 1919, p. 211.

of women, as well as men. An investigation upon the subject, made in 1873 by the United States Consul at Bucharest,[1] showed that the so-called castration of women among the Scoptz did not consist in the removal of the ovaries at all, but the mutilation or excision of the female breasts, together with the clitoris, the labia minora, and sometimes the labia majora.[2]

Spencer Wells states:[3] "Dr. Junker reports that the aborigines of Australia and of New Zealand perform ovariotomy on young girls by incision in both inguinal regions." Dr. Schlink of Sydney has investigated the matter, and he says in a communication: "Most medical students will be astounded to learn that ovariotomy was in practice many years ago by the natives of the Herbert River, Queensland." According to the researches of N. D. Miklouho (as published in the proceedings of the Linnean Society of New South Wales), this operation has undoubtedly been performed on not a few occasions. A man, who had been staying for some time with the natives living a little way up the stream of the Herbert River in the vicinity of the Parrapitshuri lake, reported that he had noticed among the natives a peculiar looking girl, who appeared to avoid the society of other females, and remained always with young men, sharing with them their occupation and toils. This girl was almost destitute of breasts, was very slim-limbed, had some hair on the chin, and altogether had a kind of boyish appearance. Two long cicatrices on the groin of the girl were explained by a native who had been some time on a station, and could speak a little English: "Alsame—spayed cow." The man explained by gestures that after the incisions in the groin were made the ovaries were torn out. Another observer—Mr. MacGillivray—saw a woman at Cape York who had undergone this operation, and convinced himself of the existence of two cicatrices in the groin. The woman was born dumb, and the operation of ovariotomy had been

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[1] *Atlanta Medical and Surgical Journal*, vol. xi. p. 483. Teinturier, "Progrès Médical," 1876, p. 917.

[2] Mann's "System of Gynæcology," vol. ii. p. 838.

[3] "Abdominal Tumours," 1885, p. 46.

performed to prevent her having dumb-born children. The operation must have been performed by the same instrument, namely, a hatchet-shaped and sharpened piece of stone, which is used for the performance of the "mikae" operation, which has been largely done among the natives of Australia. It would appear that after the incisions have been made through into the peritoneal cavity, the fingers are inserted and the ovaries brought out of the wound and pulled off. This tearing rather than cutting away of the ovaries probably accounts for the non-occurrence of fatal hæmorrhage.

We now come to those instances which have been recorded in which an ovary has been removed during an operation, either because it was found in a hernial sac, or because it was encountered during the course of an operation undertaken for another object.

Frank de Frankenau [1] has recorded that an ovary that had become herniated through an accidental wound in the abdomen, was successfully removed.

Laumonier (1776), whilst operating on a pelvic abscess, encountered an ovary which he removed, thinking it might be in a scirrhus condition. The patient recovered.[2]

Percival Pott removed both ovaries from a young woman, and Lassus [3] saw a surgeon remove one by mistake in the case of a girl of eighteen; the surgeon did not, however, know that he was operating on an ovary until he had removed it.

Percival Pott's case [4] is well known, and has been often quoted. The woman, who was about twenty-three years of age, was taken to St. Bartholomew's Hospital on account of two small swellings, one in each groin. These lumps were painful, so Potts made an incision over the hernia, when

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[1] "Satyræ Medicæ, Satyra 2 de castratione mulierum"; the author was born 1643 and died 1704.

[2] Boinet, "Histoire de la Société Royale de Médicine," 1782, tome v.

[3] *Pathol. Chirurg.*, t. ii. p. 100. Deneux and Englisch have recorded cases when the ovary has been removed more or less without any direct intention by the operator during the course of an operation.

[4] "Chirurgical Works of Percival Pott," F.R.S., London, 1783, vol. iii. p. 329.

" a fine membranous bag came into view, in which was a body, so exactly resembling a human ovarium that it was impossible to take it for anything else ; a ligature was made on it, close to the tendon, and it was cut off. The same operation was done on the other side. . . ."

We now pass to the observations of James Blundell, of Guy's Hospital, a man, as Grieg Smith rightly remarks, who was far in advance of his time. Blundell pointed out very clearly that operations on the Fallopian tubes and ovaries were quite within the legitimate province of surgery, and he also pointed out that the removal of the ovaries would relieve dysmenorrhœa. However, he never carried his suggestions into practice.

In 1825, Blundell read a paper before the Medico-Chirurgical Society of London, entitled " A Contribution of Experiments and Observations on Injuries of the Belly, considered in the Relation to Abdominal Surgery," and he deduced the following conclusions :

1st. That the generally received opinion, that inflammation in a spot of the peritoneum will almost invariably diffuse itself over the greater part of that membrane, is unfounded on truth.

2nd. That extension divisions of the peritoneum are not of necessity fatal, that the womb, spleen, and ovaries may be taken away without necessarily destroying life.

Blundell then adds some remarks on the extirpation of the healthy ovaries :

> This operation, even granting it to be safe, can scarcely in any instance be necessary, though it may be observed, by the way, that it would probably be found an effectual remedy in the worst cases of dysmenorrhœa, and in bleeding from monthly determination in the inverted womb, where the extirpation of the organ was rejected.

From Blundell we pass to Koeberlé, not taking into account for the present, the cases of ovarian tumours that were removed in all countries during this period, as ovariotomy will be considered in another place.

Koeberlé, in 1869, wishing to reduce a retroversion of the uterus by suturing the broad ligaments to the abdominal

walls, removed an ovary during the course of the operation, because it stood in his way.[1]

Esmarch, a little later, removed both ovaries in a case of mal-development of the vagina, because at each menstrual period they descended into the inguinal canals and caused general suffering.[2]

Then we come to Tait's operation that we related above, when in February, 1872, with complete success he removed an ovary the size of a pigeon's egg, which, he says, contained a chronic abscess. This, he claimed, was "the first record in the history of surgery of the removal of a small ovary on account of pain."

In 1878, Professor Hegar, of Freiburg, published for the first time, in his work "Die Castration der Frauen,"[3] an account of his first operation. He operated on July 27, 1872, removed both ovaries on account of pelvic pain and dysmenorrhœa, and found the ovaries diseased. The patient died of sepsis.

Lawson Tait, encouraged by the success that had attended the operation on Mr. Hallwright's patient, determined to remove both ovaries for the purpose of arresting menstrual hemorrhage of a perfectly intractable character, on a woman aged forty, who had been for some months under his care. The result was a complete success. The patient was alive and well in 1882. This operation was performed on the first of August, 1872, five days after Hegar had performed his first operation.

On August 17, 1872, Battey, of Rome, America, operated on his first case.

In an article by Battey and Coe,[4] the claim of Battey is put forward in the following words:

> With Battey the discovery of this operation was the result of mature and deliberate thought. As early as 1866, he was charged with the care of a young lady aged twenty, apparently perfect in her physical organisation, save only in the absence

[1] *Gaz. de Strasbourg*, 1877, n. 3, p. 28.
[2] *Arch. fur Gynaak*, t. xxii. p. 132, 1874.
[3] Leipzig, 1878.
[4] "The American System of Gynecology and Obstetrics — Gynecology," vol. ii. p. 840. The passage appears to have been written by Battey himself, as he adopts a similar vein in another paper.

of her menses. The menstrual molimen has recurred at the usual intervals without any show of menstrual blood. The nervous and vascular disturbances which attended upon the failing function has broken down and destroyed her health, and she died without relief. To Battey, the untoward result of this interesting case was most unsatisfactory. His investigations showed an absence of uterus, there being but a small rudimentary nodule to represent that organ. He reasoned with himself that the fatal issue of the case was due to the functional activity of the ovaries, uncompensated by the normal issue of blood. He said to himself, " If I could but remove her ovaries, stopping the function of ovulation, the balance would be restored ; there would no longer be need of the function of menstruation." He diligently and anxiously searched the records of medicine for light upon the subject, but he searched in vain for any effective remedy to control the diseased condition, or for any precedent for the extirpation of the ovaries in such circumstances. Feeling within himself great dissatisfaction with the paucity of our resources under the circumstances, he inwardly determined that another such case should not die on his hands without essaying the extirpation of the ovaries in the hope of rescue.

It was not long after this fatal issue in this case that there came under his observation, a young lady with persistent and long-standing amenorrhœa, whose general health was wrecked and broken down by a similar condition of unrelieved menstrual molimen. After long and fruitless efforts to establish the menses and to relieve the sufferings of the patient, he was forced to offer her the extirpation of her ovaries as the only remaining hope of her restoration to health. The proposal was made to the patient and her friends as the naked creation of his own brain, and it was distinctly stated to them, that it was unsupported by any practical test and wholly unsupported by the authority of any recognised medical opinion. The patient was warned that she might encounter as a result of the operation the loss of many of the feminine graces ; that a masculine change in her voice was possible ; that a sprouting of beard upon her face was possible ; that she might confidently expect to be completely unsexed by the operation. So extreme were her physical sufferings and her utter despair of other relief, both she and her family seized with avidity the proffered alternatives in the operation.

On August 17, 1872, at Rome, Ga., the operation was done whilst she was already experiencing the prodrome of a coming paroxysm. The patient having been chloroformed, the abdomen was opened three inches in the median line. The pelvis was now explored with the fingers of the left hand. The uterus in respect to its size, position, and texture appeared to be perfectly healthy. The ovaries likewise

appeared healthy. There were no abdominal adhesions of the pelvic organs. To the right of, and behind, Douglas' fossa was a fibrinous deposit, probably the remains of abscesses in that region which had discharged by the rectum and vagina. The right ovary was quickly brought to the light between two fingers and examined. Upon its upper and posterior surface was distinctly marked a freshly ruptured graafian vesicle, and a small drop of yet liquid blood exuding from the point of rupture. It seemed as though the ovum had but just escaped. The base of attachment to the broad ligament was transfixed at the centre with a needle carrying double silk, and tied in two equal halves. The left ovary was now searched for and seized over the ramus of the pubis at the left inguinal ring. Upon its surface it presented likewise the marks of a recently ruptured graafian vesicle, with a spot of darker-coloured and coagulated blood in the point of rupture. . . . The ovaries having been previously cut away, the ligatures were cut short and the pedicle allowed to drop back into the pelvis. There was a little oozing of blood-stain serum, which was carefully sponged out, and the walls of the abdomen were brought together by three deep silver-wire sutures, including the peritoneum, and secured by compressed shot. Eight surgical sutures completed the closure. No attempt was made at any special antiseptic precautions in the operation. The patient was put to bed in one hour from the commencement of the chloroform. On the second day the weather became intensely hot, and so continued up to the fourteenth day, without a drop of rain to cool the burning earth, the thermometer ranging from 90° to 98° Fahr. in the shade. She had some septic complications, but was out of bed on the sixteenth day and made a good recovery.[1]

During the month of January, 1876, Trenholme, of Montreal, practised oophorectomy for a large fibrous growth of the uterus. Severe uterine pains and hæmorrhage were the actuating reasons for the operation; the patient recovered.

In the same year (1876) on August 2, Hegar removed the ovaries for hæmorrhage due to a myoma of the uterus.

We are now in a position to examine the claims of the three men, whose names have been associated with the first operations performed on the ovaries.

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[1] In the following month a detailed history of the operation was published by Battey in the *Atlanta Medical and Surgical Journal* for September, 1872, with the statement: "As far as my means of information enable me to judge, this operation is unique in the annals of surgery, the nearest approximation to it being the celebrated case of Percival Potts, which is so distinctly stated in his 'Chirurgical Works.'"

Tait's claim is that he first deliberately operated on a diseased ovary, and removed it because it caused pain to his patient; the date of the operation was February 2, 1872, and the patient lived. Hegar's operation was done for intolerable ovarian neuralgia on July 27, 1872; the patient died four days later. Battey operated on a subject because of her amenorrhœa and hystero-epilepsy; the date of the operation was August 17, 1872.

It is apparent that Tait was the first to remove the ovary with deliberate intent, but his claim has met opposition because he did not *publish* the facts until eleven years afterwards. Now, at first sight, this seems to be a point that must carry great weight. Here was Tait, just appointed to the Women's Hospital at Birmingham, and on the slightest pretext rushing into print, and yet he deliberately abstained from publishing anything about his case, even when, in 1879, he published [1] a note on the "Removal of Normal Ovaries."

He says:

> As a contribution to the history of the proceeding I should like to supplement Professor Simpson's paper by the statement that I have removed the ovaries for the arrest of hæmorrhage in cases of myoma three times, in all three with a fatal result. It will thus be seen that the operation was performed in England five days after it was first performed in Germany, and sixteen days before it was performed by Battey. The facts of my cases, but not the details or the dates, were published some time ago.

Thomas [2] in dealing with the matter says: "Lawson Tait claims to have performed the same operation for the same purpose in August, 1872, but failed to publish his case until after Hegar's report, and hence his claim cannot be officially recognised."

We may admit, then, that Tait did not publish his second operation until 1879, whereas Hegar published his cases in 1878.

Battey deals with Tait's claim in the following words:

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[1] "Removal of Normal Ovaries," *Brit. Med. Jour.*, May 31, 1879.

[2] "A Practical Treatise on the Diseases of Women," 6th edit., 1891, p. 748.

Of the claims of Mr. Lawson Tait to the origination of this operation, it is difficult to speak with certainty, owing to the ambiguity of his style of writing, at one time reporting his case as a fatal one, and afterwards making the astonishing discovery that his patient had recovered and regained her health,[1] at one time giving Battey the credit of priority in the publication of his original case,[2] and at another time, with equal positiveness, claiming this priority of publication for himself.[3]

The history of the time shows a few unmistakable facts, viz. :

1. Mr. Tait made no publication of his case prior to May, 1879, and then made but a bare mention of his three fatal cases, without any detailed history or statement of particulars

2. It is equally evident that Mr. Tait could not have foreseen any of the great results which were to grow out of his operation, or he would not have remained for nine years ignorant of the fate of his first patient, whether she lived or died.

3. Mr. Tait, having published in 1873 his Hastings prize essay upon " The Diseases of the Ovaries," certainly could not have foreseen any of these results, or he would have taken the occasion to put himself upon record, before the medical world.

Now that we have all the facts, we shall see that Battey was in error. At the International Medical Congress, held in London in August, 1881, Tait evidently handed round a list of cases; these cases, however, are not published in the Transactions, but they are referred to by himself and by others.[4] In that list, he said that his first case had recovered. Now it is evident that Battey confused this case, *i.e.* Hallwright's patient suffering from ovarian abscess, and operated on successfully, February 2, 1872, and referred to by Tait in the third edition of his " Diseases of Ovaries,"

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[1] Mr. Lawson Tait: Table of Cases at International Medical Congress, August, 1881.

[2] Tait's " Diseases of the Ovaries," 1883, p. 326.

[3] Ibid. p. 107.

[4] Leon Tissier, " De la Castration de la Femme," Paris, 1885, p. 16, says : " Lawson Tait (de Birmingham), dans un tableau de ses opérations d'oophorectomie, distributé au Congr s Médical de Londres de 1881, inscrit que sa première opération date du 2 février 1872, et qu'elle fut motivée par un abcès de l'ovaire gauche." Tait says he submitted the list to the Obstetrical Section, *vide* " Diseases of Ovaries," 3rd edit., p. 327.

p. 323, with the case operated on August 1, 1872, when "both ovaries were removed for the purpose of arresting menstrual hæmorrhage of a perfectly intractable character." But while this explains Battey's error, it is indeed difficult to explain Tait's statements about his second case: the case operated on for intractable hemorrhage, August 1, 1872. In the extract that we have given above from the *British Medical Journal*, Tait says that this case died, while, in 1883, he says "the result was a complete success, and I heard of the patient being alive and well in 1874."[1] In his later work[2] he again says the woman recovered, and he saw her alive in Birmingham in 1882.

Still, this case does not in any way affect the issue; we are *not* dealing with the *second* case. We are dealing with Tait's first case, and the evidence furnished by himself is that he deliberately removed an ovary for pain due to abscess, and that he did this operation in February, 1872.

But why did not Tait publish the fact within a reasonable time after the operation? We have no explanation to offer, and must admit that it seems a most extraordinary thing for Tait to have kept silent on the subject. Even at the International Congress, when reading a paper on "Recent Advances in Abdominal Surgery," in which he mentioned the new operations that he had been performing, yet he does not mention the removal of the diseased ovary from Dr. Hallwright's patient. Still, as Tissier tells us, he inserted the case in the list, which he handed round at the International Congress, and in a paper at the end of the third edition of his "Diseases of the Ovaries," entitled "Recent Extensions of Abdominal and Pelvis Surgery," he inserts Hallwright's case, and it is in this paper that his claim is made in the following words: "As far as I know, this is the first record in the history of surgery of the removal of a small ovary on account of pain." Now, it may be asked, did Tait really perform this operation? The answer is, that he would never have dared to have made this statement if it had not been true, because the operation had been

[1] "Diseases of the Ovaries," 1883, p. 326.
[2] "Diseases of Women and Abdominal Surgery," 1889, p. 196.

performed " with the assistance of Mr. Hallwright and Mr. Bennet May," and the latter being one of the best known surgeons in Birmingham, he could easily have exposed Tait if the statement had been false.

Here, then, this subject may be summed up in the following words : Tait was the first surgeon to *deliberately* remove a small diseased ovary, because he considered that the ovary was the cause of the patient's pain. The operation was successful, and was performed on February 2, 1872. We are by no means satisfied that the ovary had a chronic abscess, and Tait's statement showed that he was not sure himself. In fact, as the ovary was only as large as a pigeon's egg, it would appear that, though it was full of " thick grumous matter," it was probably only a very small unilocular cystic ovary.

Hegar, on July 27, 1872, removed an ovary in the case of ovarian neuralgia, and his case died. Lastly, on August 17, 1872, Battey performed his first operation and removed both ovaries for amenorrhœa and hystero-epilepsy with success.

Tait operated for hemorrhage due to a uterine myoma, and removed both ovaries with success on August 1, 1872.

Now, while we have decided that Tait was the first to perform the operation of the removal of a small ovary with a definite therapeutic aim in view, it is to Battey that the credit must be given of having brought this operation before the profession in such a way that it became widely known.[1]

> In strong contrast to the reticence and modesty of Mr. Tait in bringing his work in this department before the profession, stands out in bold relief the zeal and persistency manifested by Battey, availing himself of every opportunity to spread the histories of his cases before the profession everywhere, and stoutly and vigorously defending the operation against attack from every quarter.[2]

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[1] Tait's statement " that Dr. Battey, whose name it is proposed to fix upon this operation, did not operate until after Hegar and myself, and his publication was also subsequent to both of ours," must be regarded as absolutely wrong, for in the very same work in which this quotation appears, but on another page, *i.e.* p. 326, Tait says, " Dr. Battey was the first to publish his cases in defence of his proceedings—*Atlanta Medical Journal*, September, 1872."

[2] " System of Gynæcology," Mann, vol. ii. p. 840.

Marion Sims (who named the operation after Battey), Thomas, Clarence Webster, and others have all supported Battey's claim, and we can, without taking away anything from Tait, admit that Battey was the first to *publish* his cases, and to point out the physiological effects of the removal of the ovaries. He wrote :

> It is by the extirpation of an offending organ endowed with a peculiar and essential function, the performance of which function has become morbid and destructive of health, or endangering life, we cease to abrogate the function itself, and then do away with the pernicious consequences.

We should remember that it required much more courage in those days to perform such an operation than it does now. And we cannot help admiring Battey for his courage and perseverance, sticking in the most resolute fashion to his guns until the operation had become accepted far and wide by the profession. Let him have his proper share of the honour that is justly due to him.

V

Birmingham, 1872—Hastings Prize Essay, 1873

In 1872, Tait, when delivering his lectures at the Midland Institute on physiology and biology, introduced the subject of evolution and strongly advocated Darwin's views; this brought down on his head a storm of wrath and opposition.

It is difficult for us in 1922 to appreciate the abuse and opposition that had to be encountered at this period by any one who ventured to openly support the teachings of Darwin and his henchman, Huxley, while to understand the attitude that the educated mind was assuming towards the new truths of science, we must trace the changes that had taken place as the result of the changes in thought—originating with Keble and Froude—that were slowly but surely influencing the outlook of the divines of the English Church. The following sentences very aptly paint the better-class divine of the Church of England at a period some thirty years before Tait entered as a student at Edinburgh.

> Portly divines subscribed with a sigh or a smile to the Thirty-nine Articles, sank quietly into easy livings, rode gaily to hounds of a morning as gentlemen should, . . . carried their two bottles of an evening. To be in the Church was in fact simply to pursue one of those professions which Nature and Society had decided were proper to gentlemen and gentlemen alone. The fervours of piety, the zeal of Apostolic charity, the enthusiasm of self-renunciation—these things were all very well in their way—and in their place; but their place was certainly not the Church of England. Gentlemen were neither fervid nor zealous, and above all they were not enthusiastic.[1]

It was Keble and Froude, aided by Newman, who changed all this when the "Oxford Movement" began, and the

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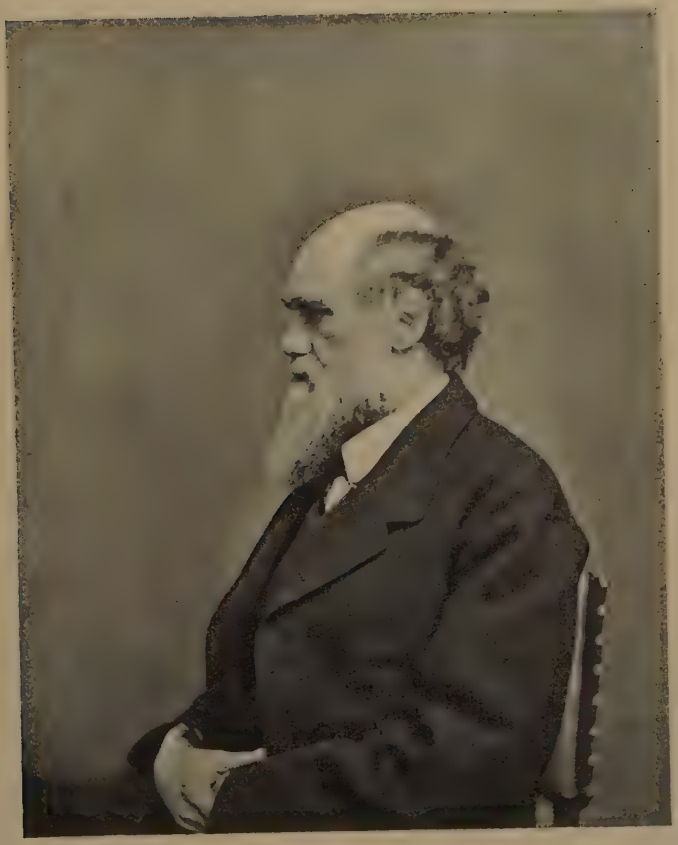
[1] Lytton Strachey, "Eminent Victorians," p. 10, 1918.

Tracts for the Times made their appearance. A new spirit was aroused in the English Church, and the sleepy, port wine drinking hunting parson in his country vicarage became aware that his reign was coming to an end, for a new Churchman had come into being, a man who was no longer content to swallow the narrow doctrines which satisfied his easy-going ancestors. The new Churchman, educated at Oxford, had become infected by the new spirit of the times, and he was no longer afraid to think for himself. The publication of the " Principles of Geology," by Lyell—which made the writing of the " Origin of Species " possible—showed that the world instead of being a few thousand years old had a history that the palæontologists could read in strata which took them back for a million or more years, and, as a consequence, the first chapter of Genesis had to be studied from an entirely new view point. This was disconcerting, and the publication of the " Vestiges of Creation " created a hubbub which had hardly quietened down when Darwin's " Origin of Species" fell like a bomb in the midst of the disputants. Churchmen of great culture and of first-class intellectual powers at first scoffed and ridiculed Darwin and his ideas, and the Churchman naturally had a great influence, and few were prepared to accept Darwin's views. This was not to be wondered at when it was found that a man like Lyell, and other well-known scientific men, as Huxley has told us, were strongly opposed to anything which tended to break down the barrier between man and the rest of the animal world. At this period the famous Oxford meeting of 1860 took place, the meeting at which the never-to-be-forgotten encounter between Bishop Wilberforce and Huxley took place, the outcome of which was that a great cause was saved from being stifled under misrepresentation and ridicule, and when Huxley had finished an exposition of the subject he turned to Wilberforce and spoke about his descent from a monkey, using words that killed the Churchman in no uncertain way : " I should feel it no shame to have risen from such an origin ; but I should feel it a shame to have sprung from one who prostituted the gifts of culture and eloquence to the service of prejudice and of

PLATE VII.



CHARLES DARWIN.

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falsehood." From that day onward the views of Darwin gained ground, but the gain was very slow. And so it came about that ten years after this meeting there were many who were ready to condemn Tait when he wished to expound and advocate the doctrine of evolution in Birmingham. But the opposition of the Churchmen had the effect of bringing forward and uniting a band of brilliant intellects, among whom were Herbert Spencer, Tyndall, Huxley, Lyell, and others, and the establishment of the *Fortnightly Review*, first under George Henry Lewes, and then under the editorship of John Morley, became a notable achievement, because the men who contributed articles to this review were men of the highest intellectual eminence, and they gradually moulded the minds of thousands—the Churchmen included—who were slowly being prepared by the advance of science and education to receive the strange truths of that new school of thought which acknowledged Darwin, Huxley, and Herbert Spencer as its guides.

Tait had been swept along by these new views and had become an enthusiastic follower of Darwin. Unfortunately his expression of his views did him harm in Birmingham, for only a limited section were prepared to accept such advanced views.[1] Reed, speaking of Tait's life in 1872, says:

> The chorus of fault finding was augmented by those who sought to stifle his evolutionary teaching at the Midland Institute at which he was lecturing on physiology and biology. But he never failed to give a Roland for an Oliver—often, be it said, when the Oliver was not worth it. To Mr. Tait, however, criticism was but a wholesome stimulus, and antagonism but an incentive to further endeavour.

It was indeed a narrow age, an age that had treated Colenso as though he were a criminal, merely because he had written what he believed to be the truth. We can,

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[1] Tait once remarked in discussing this period of his life, that views on Darwinism were received by most people with as much favour as a lecture on Sadism would be received by a meeting of deaconesses. Balfour in 1871, in his celebrated "Class Book of Botany," said: "There are no grounds for tracing back man to the monkey, or for supposing that, by gradual series of changes, animals and plants have become more and more perfect and complete."

at this distance of time, smile when we remember that Colenso told Dean Farrar that his laundress had actually declined to wash for him any more, because by so doing she lost her customers!

* * * * *

In the *Lancet* of April 13, 1872, there is a review of a small work entitled "On the Myoidema of Phthisis," by Lawson Tait, F.R.C.S.

In this small work Tait draws attention to the muscular phenomenon observable when phthisical subjects are percussed; this muscular irritability, or idio-muscular contractility, he terms myoidema, and in endeavouring to prove that it may be taken as a reliable diagnostic sign of phthisis, the reviewer considers that Tait has done good service, "if only in reviving efforts to what is, without prejudice to what Stokes and others have observed, comparatively speaking, an untrodden field of medical research."

In the *Lancet* of April 20, Tait returns to the subject mentioned in the preceding chapter, and in a short letter says that he has two cases where the uterine sound could be passed without any violence or harm into the peritoneal cavity. In one of these cases he considered there was a fistula in the fundus. Later on in this year he read a short paper entitled "Case of Utero-Peritoneal Fistula," before the Midland Medical Society.[1] In this case he had been able to demonstrate to his colleagues and the president of the society, that a sound could be passed through the fundus. Tait considered that the fistula had its origin in a rupture of the uterus during labour.

In June of this year Tait performed an ovariotomy successfully at the Women's Hospital, using catgut ligatures for the bleeding points, and dividing the pedicle with the écraseur.[2]

In August, 1872, the British Medical Association held its annual meeting at Birmingham. Mr. Alfred Baker, Senior Surgeon to the General Hospital, was the President, and Mr.

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[1] The *Lancet*, May 18, 1872.
[2] The *Lancet*, July 20, 1872.

Oliver Pemberton, who at a later period became one of Tait's most esteemed friends, delivered the address on Surgery.

Tait took a prominent part at the meeting, for not only was he secretary to the section of Obstetric Medicine, but he spent considerable time in drawing up a catalogue in which the specimens exhibited in the Annual Museum were described, and he also looked after the *Daily Journal.*

Eighteen years later, when the Association again met in Birmingham, Tait had become famous, and he was selected to deliver the address on Surgery.

In the latter part of 1872, he wrote to the *Lancet,* and championed with considerable warmth the case of Mr. Croft, a practitioner who had given a medical certificate of death, and had got into trouble with the police for so doing. Tait set forth the case of Croft in a very clear and able manner, and he had the support of the editor of the *Lancet.*

In November, finding that Spencer Wells proposed to read a paper before the Royal Medico-Chirurgical Society on the results of 500 cases of ovariotomy, Tait resolved to read a note himself at the same meeting on " A New Form of Trocar for use in Ovariotomy." For this instrument Tait claimed the following advantages over those in use at that time: That the penetrating edge cuts, but does not tear, making a wound which the tube completely fills, and from which it is easily retracted. One size of trocar will answer all purposes. The cutting point may be used as a knife to slit open smaller cysts contained in the major cyst, whose contents may be so viscid as to be unable to pass along any trocar. The *catches* adapted to the trocar were such as to obviate any possibility of its slipping, and the instrument was much handier than any other in use.

Spencer Wells in reply said that he had not yet used Tait's trocar, but he intended to do so.

At this meeting, when Tait came to discuss Wells' paper, he said that, in treating the pedicle, he was convinced that it was better to return the pedicle and close the abdomen. He admitted that Wells' results by the extra-peritoneal method of treating the pedicle were exceedingly good.

This method was adapted to those cases in which the pedicle was long, but if it were short, he preferred to use the écraseur, and close the incision completely.

* * * * *

In the first weeks of the year 1873, the medical world was much excited by the death of the Emperor Napoleon. Tait was well acquainted with the circumstances of the case, having learnt them from Sir Henry Thompson himself, who was an intimate friend, and on one occasion Tait gave us the full details, pointing out that this case was a good example of the bad luck which will come to the ablest surgeons from time to time.

The Emperor, before his arrival in England, in the early part of 1871, had been suffering from symptoms of vesical disease. He had also been affected with rheumatism, and during the Franco-Prussian War his misery had been much intensified, and at periods during the campaign he had been driven to distraction by the pain in his bladder. During the year 1872, his suffering had continued in spite of complete rest. His medical adviser, Baron Corvisart, had expressed the opinion that the patient had stone in his bladder, and after an interval Sir Henry Thompson was consulted, and he met Sir William Gull, who was the Emperor's consulting physician. At this consultation the chief point discussed was the condition of the illustrious patient's prostate, which Sir Henry declared to be healthy and normal in size. He, however, tried to persuade the Emperor to let him pass a soft catheter to see if there was any residual urine, but the patient declined the suggestion. After this some months elapsed, and then Sir James Paget was called in consultation with Gull. He advised that the bladder should be sounded for stone, but the patient would not consent. However, at a later period his suffering became so great that in December Sir Henry Thompson was called in again, and after passing a soft catheter, he found that no urine was left after micturition. On finding this he advised that an examination should be made under an anæsthetic. The Emperor consented, and in the first week of January, 1873, Clover, the celebrated anæsthetist, administered chloroform, and Thompson passed

a sound and found a large stone. He then introduced a lithotrite and measured the stone, which he found was as large as a walnut.

From this time on the Emperor placed himself in the hands of his medical advisers, and Thompson after examining the urine, which he found to deposit a layer of pus and to contain more or less blood, decided to perform lithotrity in preference to lithotomy. The operation was performed on January 2. Sir Henry crushed the stone, and removed as much of the debris as possible. That evening the patient had a rigor, and during the following days suffered great pain; the urine became charged with blood and the bladder became very irritable, and after the most profound suffering it was arranged to operate again on the sixth. Just as everything was ready the patient was seized with a rigor, and the operation was postponed for a few hours. Chloroform was then administered by Clover, and Thompson discovered that a large fragment of the crushed stone was imbedded in the prostatic urethra, and this prevented any instrument being passed into the bladder. This piece was removed after much trouble, and lithotrity was again performed. The next day it was found that the urethra was again obstructed with a fragment, but no attempt was made to dislodge this. The patient's condition did not improve much during the following days, and so a third operation was decided on; but the patient sank rapidly and expired on January 9.

A post-mortem was held by Burdon-Sanderson, when it was discovered that the ureters were dilated, and that half of the calculus, weighing about ¾ ounce, and measuring over an inch in length, *had been left uncrushed in the bladder!*

* * * * *

In the month of February, Tait read a paper before the Royal Medico-Chirurgical Society on a "Successful Case of Gastrotomy in Extra-Uterine Gestation." In this case the pregnancy was retro-uterine. The child had arrived at term, and died about the end of July, and the operation was performed by Tait in the following November. After opening the sac, the child was removed, the edge of the

wound in the sac was stitched to the edge of the peritoneal wound, the peritoneal cavity being thus excluded. The upper half of the parietal wound was closed by deep sutures and a drainage tube inserted. A foul discharge issued for the next four weeks until the great mass of the placenta was removed, after which the cavity closed.

Spencer Wells, who was present at the meeting, discussed the paper, and said that the communication was an important one, for Mr. Tait had shown that the placenta might be left to be discharged through the abdominal opening. This removed one of the great difficulties and dangers of the operation. He, however, thought that in this particular case, the incision might have been made through the posterior wall of the vagina; it would have allowed more perfect drainage, and have imitated the natural process when the fœtus was spontaneously discharged, which was usually through the vagina or rectum.

Although Tait did not reply to this suggestion made by Spencer Wells, he was not in favour of the vaginal route, because only a few months before this—in July, 1872—he had attempted to treat one of these cases by the vaginal route, and although he was able to deliver the child and to get away the placenta, yet the patient died in a few hours; [1] after that he was very emphatic in condemning the vaginal route, and in this opinion he was supported by Herman.

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In April the first number of the *Obstetrical Journal of Great Britain and Ireland* appeared. It was edited by Aveling, and in the second number, published in May, Tait contributed a paper which he concluded in the June number; its title was "Menstrual Irregularities and their relation to Diseases of the Nervous System." [2]

As the chief contributors to the first number of the magazine were men such as Robert Barnes, Braxton Hicks, Lombe Atthill, and Tilt, who were regarded as authorities

[1] "Diseases of Women and Abdominal Surgery," Lawson Tait, vol. i. p. 517.

[2] Tait puts after his name "Surgeon to the Birmingham Hospital for Women, and to the Lying-in Charity; Consulting Surgeon to the West Bromwich Hospital."

PLATE VIII.

SIR WILLIAM FERGUSSON.

To face page 58.

in gynæcology and obstetrics, it shows clearly that already the young provincial surgeon had attracted attention, otherwise he would not have been asked to contribute to the second number of this new journal.

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At the May meeting of the Obstetrical Society of London Tait read a paper on "The Diagnosis of Extra-Uterine Pregnancy." He mentioned in the course of the paper that he had seen three cases of extra-uterine gestation, and that he had operated on two; one of the operations being successful. The paper had reference to *advanced* cases of extra-uterine gestation, and Tait draws attention to two points upon which he thinks reliance can be placed in diagnosing this condition. The first point is, that during the false labour which occurs when gestation has gone to its full term, there is always a "show," and after this false labour there is always a diminution in the patient's size, due to the absorption of the liquor amnii which takes place after the death of the child. Spencer Wells was present at the discussion, and said that he had seen only one case of extra-uterine fœtation.

A few months later Tait's name became very generally known to the whole profession, for he achieved his first triumph at the Annual Meeting of the British Medical Association held in London in August, when he was awarded the Hastings Gold Medal for his essay on the "Pathology and Treatment of Ovarian Diseases."

The report in the *British Medical Journal* is as follows:—

> On August 7, Sir William Fergusson, the President, introduced the successful competitor for the Hastings Gold Medal, Mr. Lawson Tait, of Birmingham. The President said: "He had to congratulate Mr. Tait upon achieving the distinction; and the honour was the greater, inasmuch as an award was not made as a matter of course, for though in some years there had been competing essays of singular merit sent in, the medal had not always been given, while this year it had been awarded with every imaginable approbation; and that circumstances showed that the winning essay very much redounded to the credit of the writer. [Cheers.] The question proposed was one of comparatively modern date, and one on which some of the best workers

of the day had been most recently engaged. This being the fact, it was marvellous to him that the committee on these essays should have decided that Mr. Tait had achieved the distinction which entitled him to the medal. The subject was one of the most original surgical subjects of the century, for it was one which had been developed in modern surgery. It was something for Mr. Tait to have earned distinction in this way; and all present would feel themselves honoured in having the opportunity of congratulating Mr. Tait. [Cheers.] It was his (the President's) duty to congratulate Mr. Tait in the name of the Association, and to perform the task with the greatest pleasure. He proceeded to refer to the teachings of the Medical School at Edinburgh, of which Mr. Tait had proved himself a distinguished pupil; but the winning of this medal had been his greatest distinction, and he had done honour to the teaching of Sir James Simpson, who, he added, had done much for modern medicine and surgery. He concluded by wishing Mr. Tait prosperity in a career which he had so admirably begun."

Mr. Tait said a few words in acknowledgment.[1]

These complimentary words, spoken by Fergusson, who was at that time the leading surgeon in London, immediately gave Tait a standing in the profession which he had hardly dared to hope for so soon. He remembered Fergusson with gratitude, and spoke often with admiration of his great dexterity, his splendid presence, and his superb calmness and balance.

The kindly words of such a great surgeon were of great use to a young man like Tait; and Tait had one virtue, he never forgot any one who had helped him in his years of struggle. We remember well two stories that he told about Fergusson. The first one concerned Speke, the African traveller, who after his return to London, went one day to see Fergusson operate, but was so overcome by the sight that he fainted.

A few days later Speke had the misfortune to crush his finger. He came to Fergusson, who told him that it was necessary to operate on the crushed member. Speke held out his hand without the slightest tremor, and submitted to the operation.

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[1] *Brit. Med. Jour.*, August 16, 1873.

The second story was one that we heard in London before Tait related it to us. Fergusson was of a very kindly nature, and when called into consultation never gave his *confrère* a lecture at the bedside. If the family attendant had made a wrong diagnosis, Fergusson would say, as though lost in admiration of what had been done, " Dr. So-and-so, did you really do that ? Well, well, come and tell me more of your method." With that Fergusson would retire to a neighbouring room, close the door, and walking up solemnly to the family attendant, would place his hand on his shoulder and say, " Dr. So-and-so, you are a damned fool. That man has not got so-and-so, he has so-and-so ; alter the treatment at once." This kindly method of letting his less-distinguished brethren down made Fergusson a much sought-after consultant, and from the fees derived from his consulting practice, and from his enormous operative practice, he made an immense income.

VI

Pathology and Treatment of Diseases of the Ovaries
1874

Contrary to what we might have expected, Tait did not at once publish his "Hastings Essay." He waited for twelve months, and dividing it into seven instalments, he published these in the *British Medical Journal*, the first part appearing on May 30, 1874, and the last on July 11. His contribution appeared under the title, "Hastings Prize Essay, 1873, On the Pathology and Treatment of Ovarian Disease."

Later on in the same year the Essay was published by Smith, Elder & Co.

"The Essay," as published in the *British Medical Journal*, was afterwards altered and used by Tait as his chapter dealing with diseases of the ovaries in his small work on "Diseases of Women," published in 1877. Later on, in 1883, Tait greatly enlarged the Essay and published it as the fourth edition of his "Diseases of the Ovaries"; this is the volume which is best known to gynæcologists.

In the original Essay Tait first deals shortly with the anatomy and physiology of the ovary, and then passes on to consider the various diseases which the ovaries are subject to, and touches on the subject of ovarian dysmenorrhœa, and the influence of the ovary on delayed and difficult menstruation; and among other things recommends that Simpson's galvanic pessary, though not free from risk, may be tried as a therapeutic agent, because he considered that the "irritation set up by the presence of the galvanic stem in the uterus is communicated indirectly to the ovaries in a manner that is not as yet explicable, but that it has an

influence is beyond doubt, and, if it remains within bounds, it is in a large number of cases beneficial." [1]

Tait draws attention to the condition known as "*intermenstrual pain*," occurring about midway between the periods, and says that he has himself observed two cases, "but have been unable to refer them to any category." Tait refers to Priestly's observations [2] on this subject, first published in 1872, and although much has been written about this condition, now very generally known under the German title of Mittelschmerz, yet we have not much more to offer in explaining the etiology of this condition than Priestly had, who considered, "that intermenstrual pain is definitely associated with the physiological changes in the ovary, which result and end in ovulation" (Howard Kelly).[3]

Tait next proceeds to consider in his essay the displacement that the ovaries are liable to, and then goes on to deal with that "large class of ovarian diseases due to altered hæmic nutrition of the gland," and he groups them into three classes:

1. Ovarian hyperæmia;
2. Chronic ovaritis;
3. Acute ovaritis.

In dealing with ovarian hyperæmia, he recites the case of a young girl, which he again quoted [4] years afterwards to

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[1] Fifteen years later we find Tait still with some faith in the galvanic stem pessary, but by this time he had learned the danger of this instrument with regard to the Fallopian tubes, for he says ("Diseases of Women and Abdominal Surgery," p. 281): "I have had more than one painful experience to the effect that the use of galvanic stem-pessaries may lead to suppuration of the Fallopian tubes, and their use requires, as I have said, to be most carefully restricted and sedulously watched." At a later date, Hermann proved (*Med. Press and Circ.*, London, 1893, vol. lv. p. 269) that these pessaries irritate the endometrium, and an infection could spread along the tubes and set up a fatal peritonitis.

[2] *Brit. Med. Jour.*, 1872, vol. ii. p. 431.

[3] "In all the cases of 'Mittelschmerz,' in which I have had an opportunity of opening the abdomen and examining the ovaries, I have always found the tunica albuginea ovarii much thickened, and beneath this layer a number of Graafian follicles which have been unable to burst through the thickened tunica albuginea, and have consequently become small cysts. This condition of the tunica is invariably associated with dysmenorrhœa, and the number of cases in which dilation of the cervix is practised, in order to relieve dysmenorrhœa, and in which no relief whatever is obtained, can be explained by the fact that the dysmenorrhœa is due to the condition of the ovary, and not to the uterus" (McKay).

[4] "Diseases of Women and Abdominal Surgery," p. 342.

illustrate his views on this subject, as he was very keen to draw attention to the suffering that results from the routine incidental to boarding schools when young women were not allowed to rest during the menstrual week.

Concerning chronic inflammation of the ovary, he observes that, " but little is to be found in the writings of our authorities in gynæcology, and it has only been by the careful grouping of the symptoms of a large number of cases, that I have been able to satisfy myself that the condition may be accurately defined, and readily diagnosed."

It was because Tait was able to add here and there, all through his essay, these original observations on conditions that were little understood in those days, that his work was so much appreciated, and he very clearly gives details of symptoms " which have enabled me to class a number of cases together as chronic ovaritis."

Dealing with acute ovaritis in his work on " Diseases of Women," published in 1877, Tait says: " So far as I know acute ovaritis is the result of four conditions only: injury; gonorrhœal infection; septic poisoning in the parturient condition; exanthematic fevers, and acute rheumatism."

At the time of writing his essay, Tait had not yet acquired his knowledge of the effect of gonorrhœa on the Fallopian tubes, and consequently he describes as acute gonorrhœal ovaritis what in later years he would have described as acute gonorrhœal salpingitis. He had not yet learned that as a result of gonorrhœal tubal lesions, that more or less infectious material may escape into the pelvis and over the ovaries, with the result that a peri-oöphoritis can develop.

We do not find in the essay at this period the interesting observations that he gives in later works in regard to the effect of small-pox, and other of the exanthemata, in causing acute ovarian troubles.

Tait next proceeds to deal with hypertrophy of the ovary and ovarian neuralgia.

In passing, he alludes to the subject of ovarian gestation, and says he cannot admit of its occurrence, and he then goes on to make a statement that shows what a keen observer he was, for even thus early he had the genius to foresee

what it took others years to comprehend, his observation being contained in the following words: "I have had occasion elsewhere to investigate the pathology of extra-uterine gestation, and I have quite satisfied myself that all such cases are tubal in their origin."

Tait then deals with tumours that grow in the ovaries. He says that he can scarcely hope to add anything new in this essay, "but as I have had a special experience in the investigation and treatment of ovarian tumours, I shall limit myself in preference to making a mere summary of what is to be found in perfection in the recent publications of Spencer Wells, Atlee, Peaslee, and Gallez."

Tait proceeds to describe the method of diagnosing ovarian tumours, and after dealing very fully with the subject, he adds a paragraph in which thus early he shows that he had turned his attention to exploratory incision which, at a later period, was one of the advances in surgery always associated with his name.

He says:

> A final means to be employed in the diagnosis of ovarian tumours, a *dernier ressort* in case of doubt, is the exploratory incision. Such a proceeding is quite justifiable in any case of abdominal tumour which has been watched for some time, and when none of the ordinary indications suffice to make its nature clear. It is also to be recommended as a preliminary to the surgical treatment of some tumours, either uterine or ovarian, when the possibility of removal is doubtful, and the relations of the tumour can be ascertained only by exploration.

The operative treatment of ovarian tumours is then entered upon, and Tait gives very carefully the details of the method that he had adopted. He remarks that he generally operated during the afternoon, and almost invariably on the bed that the patient is to occupy. When he comes to the all-important question how the pedicle is to be treated, the influence of Spencer Wells is evident, for he says:

> I have tried four methods of treating the pedicle, namely, by the long and short ligatures, division by the écraseur, and inclusion in the clamp, and have arrived at the conclusion that the clamp is best in every way which can be applied.

> The form I prefer, after a trial of all the kinds I have seen, is the calliper clamp of Mr. Wells. I have divided the pedicle ten times with écraseur, and only one of these patients has died, the case being that of the very adherent tumour I have referred to, and which would have died no matter how the pedicle had been treated. I have a distrust of the écraseur, however; in two of my cases a pelvic hæmatocele formed coincidently with menstruation a few weeks after the operation. Mr. Wells has shown that a divided pedicle dropped back is very apt to contract adhesions to the intestines and give rise to serious trouble. One case I treated by carbolised ligatures, and she died on the fifth day from pelvic peritonitis and suppuration. In one case there was no pedicle, and I removed the tumour as well as I could, partly by the écraseur and partly by scissors, tying the bleeding points as I found them. The patient died on the fourth day. Any similar case that I may meet with in future, I shall treat by Dr. Miner's plan of enucleation. In two cases I tied the pedicle with silver wire, and dropped it back. Both these patients made excellent recoveries, and both carry their wire rings to this day without inconvenience; and I think I shall adopt this plan when I cannot use the clamp. In one case I used the long ligature, and she died on the fourth day. She will be the last in which I shall so treat the pedicle. I have had no experience in the use of cautery.

This extract is of considerable interest, because it shows that Tait, like others, was groping in the dark, first trying one method and then trying another when dealing with the pedicle. Later on, when following out the "History of Ovariotomy," we shall show that Tait in his first cases had employed the écraseur, and his results had been extremely good; but he, like others, had been impressed with the great experience that Spencer Wells had had in the operation, and he had resorted to the clamp, "and my results with it were so bad, that its employment will ever be to me a matter of bitter and lasting regret." [1]

In dealing with the treatment of the abdominal wound after ovariotomy, Tait thus early shows his attitude to antiseptics, for he says: "In my own practice, when I have treated the pedicle by returning it within the peritoneum I have always used Lister's lac plaster, and I have

[1] Tait's "Diseases of the Ovaries," 4th edit., 1883, p. 248.

found it a very convenient form of dressing. I may say, that I did not take any of the antiseptic precautions, nor have I ever seen a case where they seem to have done any good."

Tait gives, at considerable length, an account of his method of *after-treatment* in ovariotomy cases, and mentions the various complications that may arise. It is interesting to note what he says about obtaining an action of the bowels, because his views in later years were so absolutely different. He says: "In some cases the bowels may remain without action for ten, twelve, or fourteen days after the operation without giving rise to uneasiness."

In dealing with complications, Tait alludes to Marion Sims' proposal to use a drainage tube in order to prevent a collection of fluid in Douglas' pouch. Tait was not at that period in favour of the drainage tube, but in after years he became a champion of this particular method of drainage.

He remarks "that the presence of a Seton in the peritoneal cavity in every case of ovariotomy is far more likely to increase the number of cases of peritonitis, than to diminish them."

The concluding chapters of the essay are devoted to consideration of the Pathology of Ovarian Cysts, and the monograph is brought to a close by a full account of Dermoid Cysts.

Reading through Tait's Essay now, just forty-five years after it was written, one can see that he had gathered together in a concise way, all the facts then known regarding the pathological conditions of the ovary, apart from the subject of ovarian tumours, which had already been brought most prominently into the limelight because of the large number of ovariotomies performed by Clay, Keith, and Spencer Wells. But ovarian affections, apart from ovarian tumours, were but little discussed in any text-books at that time, for very little was known about the subject. The ovaries had been removed by Tait, Hegar, and Battey, in 1872, under conditions that we have related in a former chapter; but apart from these few operations nearly all the surgical work on these organs has been directed to the treatment of ovarian tumours, not to the removal of the non-cystic diseased

ovary. Yet in this very essay, Tait thus early hinted that perhaps in some cases, the question of the removal of a displaced ovary might have to be discussed, for he wrote :

> Ovaries are liable to certain displacements, which may give rise to many disagreeable symptoms, without any actual disease of the gland. Thus, one or both ovaries may, by a relaxation of their peritoneal investments, drop into the retro-uterine *cul-de-sac* and then be a source of great trouble. This will be especially the case if there be at the same time retroflexion or retroversion of the uterus, for I have known such a displacement of an ovary utterly to prevent application of any apparatus for the displacement of the uterus, and cause so much suffering as almost to make us discuss the question of ovariotomy.

An observation like the above would, no doubt, be passed over at the time as merely the suggestion of an enthusiastic young surgeon. How much more would the observation have been to the point if Tait had but published in its place the operation actually performed by him on Hallwright's patient, February 11, 1872. Here was the very case to illustrate his suggestion, for in his account of the operation which he gave years later he said : " I found the left ovary large, and very tender, displaced down behind the uterus, and the slightest pressure upon it gave rise to the characteristic sickening pain." For some reason which we cannot even guess at, Tait omitted this operation, and thereby lost from his essay something more important in the building up of his fame than all the other facts contained in his monograph.

It is evident that Tait did not accept the teaching of the pathologists of the time in regard to ovarian tumours, and his dissections of the very many tumours, removed by himself and his colleagues, enabled him to shape his own views. He says himself, that while the pathology of ovarian cysts involves a number of questions that have been raised and discussed by observers of the greatest eminence,

> still, up to the time of writing this, I have found little that has either harmonised with my own observations, or seemed to me to give any satisfactory explanation of the growth. I have had opportunities of examining a large number of tumours removed in the operation of ovariotomy, and I have come to some conclusions which are at variance with those

of many observers, but which, nevertheless, seem to me to be founded on fact, and to have at least this recommendation, that they simplify matters very much.

Tait's clear exposition of what he considered to be the origin of ovarian growths was so well considered that, writing ten years later, he said: "Since I wrote this [the Hastings Essay] a good deal has appeared on the subject, much of which I have carefully studied, but without finding reason to depart from the views I have published in 1873."

It was by his original views that Tait attracted attention to himself when this essay was published, and the essay showed him as an original observer and a practical surgeon, and before long he began to reap the benefit of his work. His name became known to many surgeons, who had not previously heard of him, and as he resided at Birmingham, and was more readily reached than if he had resided in London, his consulting practice rapidly increased throughout the Midlands. He made it his practice to attend the meetings of the various medical societies in London, and he never lost an opportunity of airing his views, particularly when he could do so at the expense of some well-known surgeon.

VII

Abdominal Hysterectomy for Fibro-Myoma

1874

At the October meeting (1874) of the Royal Medical and Chirurgical Society of London, Tait contributed a paper in which he described his first successful operation for the removal of a large fibro-myoma from the fundus-uteri by abdominal hysterectomy.

The patient, aged thirty-four, suffered from an abdominal tumour of rather rapid growth for five years. There was profuse menstruation. The tumour was removed by operation on January 16, the steps of the operation being, according to the report, " exactly the same as for ovariotomy." The tumour was removed at a point just above the internal os. Recovery was rapid, and the clamp came away on the eighth day. The tumour weighed eleven pounds.

At the meeting, Mr. Timothy Holmes was in the chair, and he inquired under what conditions such serious operations were advisable ?

Mr. Spencer Wells said that he had operated on such tumours, but he recollected only three cases in which the operation was successful, and in one of these cancer of the neck of the uterus occurred a year later. In one case " he had removed a tumour weighing twenty pounds, which was killing the patient by its presence, and the patient was well now." The mortality, Wells however considered, must always be the more serious after such operations than after ovariotomy, and " he made it a rule never to interfere unless the life of the patient was endangered by bleeding, or by pressure on the intestines by the uterus."

A member asked Wells what treatment should be adopted

in cases where severe hæmorrhage occurred from the presence of fibroids in the walls of the uterus? Wells replied that the question of gastrotomy need never be entertained, as they might always be removed by dilation of the os and enucleation, or other means.

After this, Mr. Holmes criticised the way in which the case had been brought before the Society. The paper, he said, should have contained more details and the reasons for undertaking such a serious operation; the author had contented himself with the bare statement that there were "symptoms of obstruction," and that the patient desired the operation. Mr. Holmes suggested that the danger of the operation was increased by the solid nature of the tumour, which would tend to the formation of adhesions, and the pedicle would also, in all probability, be thicker and more vascular than in ovarian tumours.

Mr. Victor de Meric blamed Tait for not at least stating some of the experiences of previous operators, and for not describing in more detail the mode of operating in such cases.[1]

Tait, for some reason, did not attend this meeting, and thus gave some of his London opponents an opportunity for unfavourably criticising his operation. It is quite possible that Tait remained away from the meeting on purpose. He knew that the operation he had performed was a very unusual one, and although it had been successful, he could hardly expect the London surgeons to hail this success with approbation. Mr. Timothy Holmes was not a friend, and Tait's criticisms of his papers on "Hospital Mortality" had no doubt made him very bitter to the young provincial surgeon.

Tait replied to his critics in a letter in the *Lancet*.[2] I have reproduced this letter because it gives a good idea of Tait's style when defending himself. It shows how he was able to pick out the weak points in his opponents' case, and it also shows that Tait never lost an opportunity of mentioning the great men who were his friends.

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[1] The *Lancet*, October 31, 1874, p. 626.
[2] The *Lancet*, November 7, 1874, p. 680.

Case of a Successful Removal of a Large Fibro-Myoma from the Fundus Uteri

To the Editor of the Lancet.

Sir,—I very much regret that I was unable to be present at the Medico-Chirurgical Society on Tuesday last, and that I therefore missed the opportunity of disposing of Mr. Timothy Holmes' criticism without trespassing on your valuable space.

Mr. Holmes seems to have made, what is to me, quite a gynæcological discovery, when he stated that the solid nature of such tumours tends to the formation of adhesions, and therefore increased the risks of the operation—that is, I presume, increases the risks on that account over that of ovariotomy. I am not aware of any published results of Mr. Holmes' practice in the removal of abdominal tumours, and therefore I do not know on what he bases his statement. But my own experience is exactly the reverse, for out of a considerable number of cases, where I have seen the abdomen opened, before or after death, in the presence of a large uterine tumour, I have never yet seen an adhesion, whilst in the case of ovarian tumours their complete absence has been almost exceptional.

My patient was of very small size, and only thirty-four years of age—that is, she had about fourteen years, or more, to live before she would reach her climacteric period, at which such a tumour might cease to grow. The tumour had been rapidly increasing in size, and at the time of operating it weighed eleven pounds; and Mr. Spencer Wells stated in the discussion that he had been obliged to remove one only nine pounds heavier in order to save a patient's life. The late removal of a solid twenty-pound tumour must be a far more formidable operation than the earlier removal of one of eleven pounds weight. These facts, even without the additional reason that the patient's life was constantly in jeopardy by attacks of intestinal obstruction, and by the increasing discomfort, pain, and exhaustion, would have been sufficient to determine me to advise the removal of the tumour; and the complete relief of all the symptoms by the operation has justified the proceeding, at least in my opinion, and in the patient's.

On the other hand, might I ask Mr. Holmes if it has been his practice in all his operations for the removal of tumours, to wait until growth has nearly destroyed his patient before he attempts a cure? In the case of a tumour in the neck, would he advise waiting till the growth had occupied all the available space, and displaced or involved all the important structures in its neighbourhood? Has he done this, then his practice has been very bad; but if he has not, then he has but scant reason for blaming me for removing a tumour

which I had diagnosed to be removable, and which I removed at a time when a cure was possible, instead of waiting till the successful result of an operation was hopeless. I have had occasion to remove an ovarian tumour, successfully, not more than six ounces in weight, because it seemed to be interfering with the patient's life, and I know of other cases like it. But ovariotomy is an established operation, while the successful removal of uterine fibroids still remains to be worked out, and the way shall not be stopped by mere alarmists.

In answer to Mr. de Meric, I may say that my paper was on a specific case, and not on previous operations devised by others. These will be considered in another paper, which I have in progress, and which I hope to be able soon to submit to the Medico-Chirurgical Society. In it, the operations of my friends, M. Pêan and Dr. Marion Sims, will be discussed to the best of my endeavour. The case, now submitted, is only one of a series, all to be published ; but it is the only one I have had of its kind—that is, the only one removed by abdominal section.

One notices how readily both Spencer Wells and Tait promise to bring forward another paper, each with further information as regards their operative experience of fibroids of the uterus ; but we look in vain during the next few years for these papers from either surgeon ! [1]

When, in a later chapter, we come to deal with the history of the operation of hysterectomy for fibroids of the uterus, we shall see that at this period abdominal hysterectomy had already been successfully practised by some surgeons in America, and by Péan in Paris, and Koeberlé of Strasburg.

1875—Birmingham Medical Institute

In the early part of 1875, Tait was very busy helping to found a Medical Institute at Birmingham.

In February, a meeting of members of the medical profession was held, presided over by Dr. Bell Fletcher, Tait's former partner.

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[1] Tait's reticence is probably explained by a sentence in his work on "Diseases of Women," 1877, p. 167 : "I have three times performed this operation, involving the removal of the whole fundus uteri, the tumours being all purely myomatous, but in only one instance have I had a successful result."

In his opening address, Dr. Fletcher urged on the profession the importance of the proposed scheme,[1] which involved :

(1) The formation and maintenance of a library of medicine, and the allied sciences ;

(2) The advancement of professional knowledge by the establishment of laboratories and museums, and by the delivery of lectures ;

(3) The relief of poor and necessitous members of the profession ;

(4) The acquisition of buildings suitable for the purposes of the association, with power to grant their use for any scientific, literary, or legal, society.

Everything went off well at the first meeting, but almost immediately afterwards a storm arose. Donations had been solicited towards the Building Fund, and the profession had responded with much liberality, but unfortunately a donation from a professed homœopath had been accepted. This was regarded as a wrong step, and the *Lancet* took up the stand that the homœopath should not have been admitted, and said : " Our opinion of homœopathy has not changed, and we distinctly maintain that it is morally impossible for the practitioners of rational medicine to hold any professional relations with professed homœopaths."

But these views did not meet with general acceptance, for many of the medical men in Birmingham, and nearly all the local daily papers, seem to have declared for the homœopaths. One of the medical men, who was the most conspicuous in his opposition of the homœopaths, was Oliver Pemberton, a well-known surgeon of Birmingham, who became a very firm and dear friend of Tait in after years.

The controversy lasted until the end of May, when the first Annual Meeting of the Institute was held. Mr. Alfred Baker moved the adoption of the report of the committee, and Mr. Oliver Pemberton proposed an amendment disapproving the action of the committee in admitting professed homœopaths as members. The amendment was lost, and so the friends and admirers of homœopathy were highly gratified.

[1] The *Lancet*, February 20, 1875, p. 287.

This innovation by the medical men of Birmingham was only the first of many important steps, which were taken to show the rest of the profession that there was growing up in that city a desire to assert its own opinions on many important questions in medicine, and when, year after year, Tait, Savage, and others, sent forth their contributions to surgery, and amidst the greatest opposition, gained a slow and grudging acknowledgment of their views, then it was that the phrase " Birmingham School " sprang into existence. In some cases, it was used in order to back up a view, in other cases, especially when used by London surgeons, it was merely used in a contemptuous sense ; just as, at one time, the term " brummagem " was used to denote the cheap and bizarre goods manufactured in Birmingham. But the day came when the goods turned out of the factories of the Midland capital were acknowledged the equal of any manufactured at any centre in England, and no longer did people think of Birmingham as the fount of cheap trinkets. So also did it come to pass that as the years went on, and the wonderful results achieved by Savage and Tait in abdominal surgery became known, and acknowledged, opponents ceased to use the term " Birmingham School " in a contemptuous way, and readily acknowledged that the school represented the most advanced and important teaching centre for the new gynæcology and abdominal surgery. Needless to say that Tait was always very proud of the share that he had taken in the founding and development of the *Birmingham School.*

One thing was characteristic of the Midland centre, and that was a liberality of mind and an honest endeavour to adopt new ideas, and cast aside narrow and provincial habits. Thus Pemberton set a good example after he had opposed the admission of the homœopaths to the Medical Institute, for he wrote to the *Lancet* and said :

> I cannot but recognise the fairness and wisdom of the comments made by the *Birmingham Daily Post* on the result of the recent meeting, and I take this opportunity to say, in accordance with the spirit of these observations, that the Institute may yet find myself, and I trust those who have acted with me, are amongst the number of its best friends.

VIII

Diseases of Women

1877

In the early months of 1877, Tait published a small work on the "Diseases of Women." [1]

In the Preface the author says that "the chief object of this book has been to offer results of my own experience in as condensed form as possible; and I have therefore avoided, as far as I could, the long quotations, needless references, and detailed accounts of cases." He draws attention to the fact that the chapter on the Diseases of the Ovary is an enlargement of his Hastings Essay of 1873.

Tait divided his work into eight sections. In the first he deals with the affections of the Mons Veneris; in the second, with those of the Vulva; in the third, Diseases of the Vagina, Urethra, and Bladder; the fourth is devoted to the Uterus; the fifth to the Broad Ligaments; and the other sections to the Fallopian Tubes, Ovaries, and Pelvic Bones.

Passing over the first five chapters, which contain a sketchy *résumé* of the diseases of women, augmented by the individual experience of the author, we turn to the sixth chapter, which is devoted to the "Fallopian Tubes and Fimbriæ."

We can ascertain from the opening sentence of the chapter that Tait had not yet seen a hydro-salpinx, for he says that "destruction of the tubal-epithelium may cause atrophy and occlusion of the tubes, and occlusion of the apertures of the tubes may be the cause of another disease of the tubes which is described, but which I have never seen, dropsical distension."

Tait then goes on to say, that he wishes to put forth some new views as to the pathology of extra-uterine gestation, which he hopes will greatly simplify the confused subject,

[1] Williams and Norgate, London, 1877.

and draws attention to the fact that Parry, of Philadelphia, had favourably considered his views, and, to a certain extent, had adopted them in his recent work on "Extra-Uterine Gestation." Tait then expounds his now well-known proposition that "every case of extra-uterine pregnancy is tubal in its origin, and that it may become intra-peritoneal, or extra-peritoneal, just as the tube happens to burst; the instances of ovarian pregnancy are the exceptions to this rule."

In this chapter on the Fallopian Tubes, we look in vain for any mention of pyo-salpinx; but in the next chapter we see that Tait was beginning to recognise that gonorrhœa was a potent cause of trouble, for under the heading of Acute Ovaritis he places gonorrhœa as a cause, and the description that he gives of a patient suffering from *acute gonorrhœal ovaritis*, will be found in later years applied by him to one suffering from "acute gonorrhœal salpingitis"; but this knowledge was to come to Tait at a later date.

In this chapter he draws attention to the original observations that he had made on the occurrence of acute ovaritis in certain of the exanthemata. In 1870 and 1871, and still more in 1874, his attention had been drawn to the occurrence of acute pelvic peritonitis in women after attacks of scarlet fever, and small-pox; these attacks leaving indications which showed that the mischief began in the ovaries. He came to the conclusion that the changes in the ovary were similar to those taking place in other parenchymatous organs during scarlatina, and believed that the degeneration of such a number of follicles results in more or less impairment of the function, sufficient in some cases to affect the reproduction faculty.

Skrobansky [1] investigated the condition of many ovaries removed from women and children dying from scarlatina, diphtheria, typhoid fever, and miliary tuberculosis. He found that in all cases the ovaries had undergone more or less degeneration, but its character was the same, no matter what was the form of infection. Furthermore, neither the

[1] *Jour. d'Obst. et de Gyn.*, October, 1901.

intensity nor the duration of the diseases made any difference in the extent of the affection.

The result of Tait's observations, and of later observations, shows that infectious diseases do produce changes in the ovaries, and that these changes are exactly the same as those produced by these diseases upon other parenchymatous organs, and Hirst has recently shown [1] that the ovary may be so affected in typhoid fever, that an abscess may form; while Dirmosen has shown that the uterine appendages may be inflamed during the course of enteric fever.[2]

Passing over Tait's observations on the other diseases that the ovaries are subject to, we at length come to the operations on ovarian cysts, and note that he gives an illustration of his trocar, which he alludes to in the following words:

> The operation of tapping is best performed by a trocar, which I have devised, having a steel point with a chisel edge, which is almost blunt. The patient having been placed in a convenient position, a puncture is made by an ordinary lancet into the cyst, and the trocar is made to follow the track of the lancet. The trocar is so simple that it never can be out of order; it forms a solid rod, which is extremely useful as a probe, and its point is sharp enough to penetrate an inner cyst, and yet so blunt as to be incapable of mischief, save in the hands of the clumsy, or the careless.

Tait then comes to consider the treatment of the pedicle, and we shall give his views at length, because, as we shall be able to show later on, his mortality varied with his treatment of the pedicle, and that as soon as he knew how the pedicle should be treated, from that moment modern abdominal surgery started. First let us insert a few lines from a later work, for in 1883 [3] he writes thus:

> When I began my practice in 1867, I employed the écraseur, a variety of the intra-peritoneal treatment, and my results over a limited experience were extremely good. Like others, however, I was so impressed with the overwhelming experience of Mr. Wells, that I resorted to the clamp, and my

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[1] *Jour. Amer. Med. Assoc.*, February 11, 1905.
[2] *Centrbl. f. Gyn.*, 1904, vol. xxviii. p. 1177.
[3] "Diseases of Ovaries," p. 248.

results with it were so bad, that its employment will ever be to me a matter for bitter and lasting regret.

In the work under consideration his practice, up to 1870, is thus described in his own words :

Having got the tumour fairly out, the surgeon's first duty is to examine the pedicle and decide how it is to be treated. This is far the most important question about the operation, and one which is by no means yet settled ; at one time I often used the écraseur, and of eleven cases so treated, nine recovered. I have a distrust of the écraseur, however, because in two of my cases, intra-peritoneal pelvic hæmatoceles formed coincidently with menstruation a few weeks after the operation. In neither of them was there any danger to life produced by the hæmatocele ; but considering how frequently an irregular menstruation occurs a day or two after ovariotomy, I have given up the use of the écraseur in the treatment of the pedicle, dreading an unfortunate result. There is, moreover, the objection to it that the raw surface of the divided pedicle is apt to become adherent to intestine and be an after source of danger. . . .

I have used the clamp a large number of times, and have had quite the average success with it. But Dr. Keith's splendid result with the cautery have induced me to adopt that method of treatment. The question really lies between the intra-peritoneal method of treatment of the pedicle and the extra-peritoneal method. In the former, the risks are, firstly, hæmorrhage ; secondly, suppuration of the stump, an occurrence which was fatal in a case upon which I operated for my friend, Dr. Boddaert, at Ghent, in 1873, and in which I used the écraseur ; and thirdly, the attachment of the pedicle to intestine. In the extra-peritoneal method, the immediate risks are much greater, and therefore it is that the success of the intra-peritoneal method excels that of the others. These risks are, firstly, invasion of the peritoneum by decomposing discharges, or by wandering and infecting cells, along the track of the pedicle ; for sometimes when the pedicle is short, thick, or broad, it is a very difficult matter to close the integuments accurately round it ; and on post-mortem examination, a minute aperture is discovered which has caused the fatal issue. Secondly, there is the risk of an abscess forming in the pedicle below the clamp or within the abdomen, as in a case operated on by another surgeon to which I was called, and wherein I fortunately discovered such an abscess, and by opening it probably saved the patient's life. There is also the further risk of gangrene of the pedicle starting from the clamp, an occurrence which carried off one of my

patients on the seventh day. Thirdly, there is the distant risk of intestinal protrusion at the seat of the pedicle, a condition not likely to prove fatal, but still to be entertained as a risk, and there is also the danger, which is both immediate and distant, of the strangulation of intestine between the pedicle and the abdominal wall. This did actually occur in one of my cases, but none of the symptoms suggested the possibility of such an occurrence, so that I did not venture to re-open the wound, as, of course, should be done if there be reason to suspect it. The balance of *a priori* reasoning is therefore on the side of the intra-peritoneal method; and though we have the splendid experience of Mr. Wells in inducing him to advocate the clamp whenever it can be used, we have the greater success of Dr. Keith undoubtedly in support of the cautery. This important question can be settled by experience only.

Of the intra-peritoneal methods, I give preference to the cautery, or the écraseur, to the exclusion of a short ligature, or the twisting plan of Professor McLeod. Of the former two, I think it likely that the cautery will be found the better. For the extra-peritoneal method I prefer the calliper-clamp of Mr. Wells to all others.

One case I treated by carbolised ligatures, and she died on the fifth day from pelvic peritonitis and suppuration.

Later on in this chapter, in dealing with the technique of ovariotomy, Tait says: "The closure of the wound I usually effect by silver sutures passed by a handled needle."

He makes some observations on the germ theory and antiseptics, which show that he was anything but a follower of Lister.

> If the stories about the much vaunted antiseptic treatment, which we hear, *but which is not published*, be true, abdominal section under the carbolic spray is by no means either a simple or a safe proceeding: and when its advocates can perform fifty ovariotomies, aided by its hindrance, and have only six deaths, then will be the time to listen to them.

Concerning the after-treatment, Tait calls attention to the necessity of making strict observations on the temperature, and says: "Nothing has been more instructive to me than a comparison of a group of such charts; and I have repeatedly seen grounds for a prognosis in a case by the comparison of its temperature range with those of former cases."

Tait notes that "Flatulence is often a very distressing

symptom, and if accompanied by a high temperature, pathognomonic of peritonitis . . . the passage of an O'Beirne's tube as far as possible up the rectus will sometimes give much relief."

He still follows the practice that he advocated in his "Hastings Essay" of keeping the bowels bound up, for he says: "the bowels may remain without action for ten, twelve, or fourteen days after the operation, without giving rise to any uneasiness."

The concluding chapter of the work is a short one on the pelvic bones, and contains nothing of any importance.

The reviews of Tait's book that we have read were not at all enthusiastic.

The *Lancet* [1] says that

> the work is generally sketchy, when it should be full and complete, and comparatively lengthy when the subject might be dismissed in a few lines. Thus symptomatology, prognosis, and treatment, are given but very briefly, while an obscure point in pathology of no practical interest is discussed comparatively fully. To students the work will give but an imperfect idea of our present knowledge of diseases of women; to practitioners we cannot think it will be of much use; while it may prove of interest to those who pursue gynæcology as a special study. We closed the work with a feeling of regret that at a time when so much had been done in, and so many were working at, the subject, any one had undertaken to write a book on diseases of women from the results of his own experience alone.

The estimate that the *Lancet* formed of Tait's work was not a correct one. The book had its influence, and it found readers and followers not only in England, but also in America, who were ready to follow its teaching. Many years later Reed spoke of it in the following words:

> This small volume came to your speaker when he was practising his profession in an isolated village in a Western State. He had been taught to apply caustics, to replace the uterus, to adjust pessaries, and to do divers and sundry other things, which in the aggregate made up the gynæcology of that period. He had faithfully followed the teachings of the day—and had become convinced of their futility. The first

[1] The *Lancet*, December 1, 1877.

> ray of light came to him in the little book from Birmingham —the darkness began to disappear. Never was a book studied with more zeal—a zeal which, a few years later, led him across the Atlantic to Birmingham to see something of the new surgery at the hand of him who had fashioned it.

It is of interest to note *en passant* that at this period, Tait was very friendly with Spencer Wells. This book is

DEDICATED TO
THOMAS SPENCER WELLS,
IN ADMIRATION OF THE BRILLIANT WORK
BY WHICH THE ART OF SURGERY
HAS BEEN ENABLED MATERIALLY TO PROLONG
HUMAN LIFE.

On p. 273 of the same work, Tait says :

> To Mr. Spencer Wells must be accorded the credit of having placed ovariotomy in the position, not only of an acknowledged operation, but of one of the most successful of one of the great operations of surgery. I do not believe any other capital operation would bear the same searching scrutiny as Mr. Wells' cases have been subject to, and come out with as brilliant a result.

Tait told us that he sent the dedication that he proposed to insert in his work to Spencer Wells, and Wells was not at all satisfied with the wording of it, so he wrote the one given above. Years afterwards, when Tait and Wells were at war, the latter said on one occasion that Tait had had the audacity to dedicate one of his works to him ; Tait thereupon looked up the correspondence which he had had with Wells on the subject, and showed in a letter to the *British Medical Journal*, that Wells had in reality written the dedication for himself. This was a *coup* that would be balm to Tait's soul. To be able to dumbfound an antagonist by that antagonist's own written words, was a method of procedure that Tait excelled in.

IX

Hospital Mortality

1877

In June of 1877, Tait published his "Essay on Hospital Mortality," [1] based on the statistics of the hospitals of Great Britain for fifteen years. Under his name on the title-page he put, "Fellow of the Statistical Society."

In his preface he tells us that the discussion on "Hospitalism," raised by Sir James Simpson, was unfortunately cut short by his death, but his papers were placed in Tait's hands by Simpson's son; he found, however, so little that had not been used by Simpson that he felt that any further investigation of the subject must be prefaced by research in another direction.

Tait, after giving much thought to the subject, came to the conclusion that the first step in future investigation was to establish the facts of a total hospital mortality for a definite and extended period. Accordingly he made in 1871 an attempt to collect details from every hospital in Great Britain for the preceding decade. The results obtained, however, were such that he came to the conclusion that they could hardly be depended upon for accuracy, and in some respects they were so meagre as hardly to be worth the trouble involved in collecting them.

He accordingly waited five years. During this time he had become known to many of the profession through his Hastings Prize Essay, and at the end of that period he made a second and much more successful attempt to obtain the data which would make his work possible. He regarded his effort in the light of a preliminary inquiry, and having ascertained the facts of hospital mortality, he considered

[1] Churchill, London, 1877.

that the next step was to inquire into the causes, or explanations of excess, and then the remedies would become apparent. He said that he thought that his figures showed incontestably that a most stringent inquiry was demanded as to the possibility of reducing the number of deaths in at least some of the institutions devoted to the sick poor.

Let us leave Tait's book for a moment, and glance back. "Hospitalism" was not a new subject of discussion when this term was first applied to it by Sir James Simpson in 1869. He gave no exact definition of the term, but in his own words it may be said to mean "the hygienic evils which the system of huge and colossal hospital edifices has hitherto been made to involve." The long controversy, in which Simpson bore so prominent a part, was but the outgrowth of the discussions raised by Florence Nightingale's work in the cause of hospital reform after the Crimean War. The general feeling created after her views were published, was that all the old hospitals should be pulled down, and new ones erected. An inquiry was made by Bristowe and Holmes in 1863, and a report published after they had inspected both the British and foreign hospitals; and six years later, Simpson declared his extreme view and advocated a system of hospitals made up of small cottages, grouped together, with one, or not more than two, patients in each room; these cottages to compose a village, and to be built "of iron instead of brick or stone, and taken down and rebuilt every few years." This was his remedy for the evils which appeared to be so evident and to be necessarily connected with the size of the hospital; and he supported his propositions by statistics showing the relative mortality in private practice and small hospitals in the country, and in large hospitals in the cities.[1]

Erichsen[2] took up the subject in 1874, and he defined "Hospitalism" to be a term which means "the general morbid condition of the building, or of its atmosphere, productive of disease." He said that while septic diseases

[1] Buck, "Reference Handbook of the Medical Sciences," vol. iii. p. 698.
[2] "Hospitalism and the Causes of Death after Operations," London, 1874.

may be met in private practice, yet they are unquestionably infinitely more common in a hospital, and when a hospital had become " pyæmia stricken " it ought to be destroyed. Here are his words :

> The town free from infection ; the hospital saturated by it, to such an extent as to induce its own surgeons to recommend their patients not to enter it, to compel them to refrain from operating, and, after every attempt that science and humanity could suggest—every hygienic means employed in vain in the fruitless attempt to eradicate the pestilence from the very fabric itself—to cause the governors, as a last resource, to decide on the demolition of the building and its complete reconstruction, at a great expense, as the only remedy. The truth is that, when once a hospital has become incurably pyæmic-stricken it is as impossible to disinfect it by any known hygienic means, as it would be to disinfect a crumbling wall of the ants that have taken possession of it, or an old cheese of the maggots which have generated in it.

Similar views were advocated in America during the ten years after the Civil War. Wooden barracks and tents were recommended for all hospitals, for the reason that huts and tents are more readily disinfected than permanent buildings ; and it was said that all of them were liable to become saturated in time with the contagion of disease, and a wooden hut may be more readily abandoned, or destroyed, than a building of brick or stone. And here let us pause to bring into the history of " Hospitalism " one great name, and the bearer of that name was Florence Nightingale.

Florence Nightingale passed much of her life as an invalid and yet did an enormous amount of mental work.

Tait spoke of her as one of the greatest women that the world had ever known. He alluded to the fact that being a woman, born in the best social rank, she had quite early concluded that she had a mission in life, and that mission was to help the suffering. She brushed aside the attractions of the social world and sought an outlet for her energies by becoming the superintendent of a charitable nursing home in Harley Street. Then the Crimean War broke out, and news came of the deplorable condition of the soldiers after the battle of Alma. Having considerable influence at the

War Office, she was appointed to go to the seat of war and take with her some forty nurses. She arrived at Scutari—a suburb of Constantinople—and found that the medical arrangements had completely broken down, and that there was no organisation of the hospital, which was overcrowded as the result of pouring wounded men in after the battle of Alma, followed by the disasters at Balaclava and Inkermann. Then the true greatness of Florence Nightingale sprang into life. She had a genius for organisation. She set to and fought tooth and nail against the opposition that met her on every hand. The wounded poured in from every side. The injured, the sick, and the dying came with a never-ceasing flow, and they had to be huddled together into the Scutari Hospital. Tait painted the horrors of that hospital as he had heard it from her own lips. We cannot give his exact words, but they were recalled to us recently when we read the following passage from the pen of Lytton Strachey: [1]

> Want, neglect, confusion, misery—in every shape and in every degree of intensity—filled the endless corridors and the vast apartments of the gigantic barrack-house, which, without forethought or preparation, had been hurriedly set aside as the chief shelter for the victims of the war. The very building itself was radically defective. Huge sewers underlay it, and cess-pools loaded with filth wafted their poison into the upper rooms. The floors were in so rotten a condition that many of them could not be scrubbed; the walls were thick with dirt; incredible multitudes of vermin swarmed everywhere. And, enormous as the building was, it was yet too small. It contained four miles of beds, crushed together so close that there was but just room to pass between them. Under such conditions, the most elaborate system of ventilation might well have been at fault; but here there was no ventilation. The stench was indescribable. "I have been well acquainted," said Miss Nightingale, "with the dwellings of the worst parts of most of the great cities in Europe, but have never been in any atmosphere which I could compare with that of the Barrack Hospital at night." The structural defects were equalled by the deficiencies in the commonest objects of hospital use. There were not enough bedsteads; the sheets were of canvas, and so coarse that the wounded

[1] "Eminent Victorians," 1918, p. 129.

men recoiled from them, begging to be left in their blankets ; there was no bedroom furniture of any kind, and empty beer-bottles were used for candlesticks. There were no basins, no towels, no soap, no brooms, no mops, no trays, no plates ; there were neither slippers nor scissors, neither shoe-brushes nor blacking ; there were no knives or forks or spoons. The supply of fuel was constantly deficient. The cooking arrangements were preposterously inadequate, and the laundry was a farce. As for purely medical materials, the tale was no better. Stretchers, splints, bandages—all were lacking ; and so were the most ordinary drugs.

To replace such wants, to struggle against such difficulties, there was a handful of men overburdened by the strain of ceaseless work, bound down by the traditions of official routine, and enfeebled either by old age or inexperience or sheer incompetence. They had proved utterly unequal to their task. The principal doctor was lost in the imbecilities of a senile optimism. The wretched official whose business it was to provide for the wants of the hospital was tied fast hand and foot by red tape. A few of the younger doctors struggled valiantly, but what could they do ? Unprepared, disorganised, with such help only as they could find among the miserable band of convalescent soldiers drafted off to tend their sick comrades, they were faced with disease, mutilation, and death in all their most appalling forms, crowded multitudinously about them in an ever increasing mass. They were like men in a shipwreck, fighting, not for safety, but for the next moment's bare existence—to gain, by yet another frenzied effort, some brief respite from the waters of destruction.

In these surroundings, those who had been long inured to scenes of human suffering—surgeons with a world-wide knowledge of agonies, soldiers familiar with fields of carnage, missionaries with remembrances of famine and of plague—yet found a depth of horror which they had never known before. There were moments, there were places, in the Barrack Hospital at Scutari, where the strongest hand was struck with trembling, and the boldest eye would turn away its gaze.

Miss Nightingale came, and she, at any rate, in that Inferno, did not abandon hope.

The genius of Florence Nightingale overcame all these difficulties in the end, and when the war was over she returned to England to receive the homage of the nation. But her health was now broken, and for years she remained almost on the brink of the grave, yet in spite of her delicate

state of health she did an enormous amount of work, and after a period she published her celebrated book "Notes on Hospitals," and at one stroke revolutionised the theory of hospital construction and hospital management. Soon after (1860), the Nightingale Training School for nurses at St. Thomas' Hospital was opened, and from this we may date the beginning of modern nursing. At first this was necessarily imperfect, but when a few years after the genius of Lister opened the eyes of the authorities of the hospital, the good work of Florence Nightingale became the handmaid to the glorious reforms that Lister's genius brought about in the stricken institutions, and to-day we have the modern hospital in all its glory.

But in the times we are dealing with, in all towns and in all countries, the condition of the hospitals was unsatisfactory, and Cadge, a well-known surgeon, said on one occasion: "I came to the conclusion in my own mind that pyæmia, if it does not find a birthplace, does find its natural home and resting-place in hospitals; and although a hospital may not be the mother of pyæmia, it is its nurse." [1]

Let us now return to Tait's Essay. As we have seen, Tait's attention was first directed to Hospital Mortality when, after Simpson's death, the latter's papers on "Hospitalism" were placed in his hands. At that time he came forward and contributed a paper to the *Lancet* entitled, "A Consideration of the Criticism advanced on Sir James Y. Simpson's Papers on 'Hospitalism,' specially those of Mr. Holmes." [2] In this letter he condemns the use of the term "Hospitalism," and says: "Certainly the name 'hospital' ought not to be used as a root from which to coin a word that, in popular parlance, is now a term of reproach. For this reason I shall discard the term;" and it is for this reason, no doubt, that his essay was entitled "Hospital Mortality," not "Hospitalism."

Tait, in the first pages of his "Essay," maintains that in an inquiry into hospital mortality it must be definitely borne

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[1] The *Lancet*, 1874, vol. i. p. 338.
[2] The *Lancet*, April, 1871, p. 443.

in mind that there are three steps in the process, which although inter-dependent, must be kept distinct. The first is to ascertain what hospital mortality really is; and this must be done on some uniform and general plan, and without prejudice. He considers that nothing had so far been written that was sufficiently precise to satisfy the wants of statisticians; and it was by statistics alone that the first of the three steps could be made.

Having established what hospital mortality really is, the next step is to inquire into the causes of its fluctuation; and the third is to discover remedies for the defects laid bare.

He announces that the main object of his essay is with the first of these steps. He says that he has made an attempt to tabulate hospital mortality on a uniform plan, and he wishes it to be understood, that if he has not been successful in obtaining exact results it was not from any want of having taken a vast amount of trouble, and for having exercised every care and caution. Tait's method of collecting information was to send circulars to over four hundred hospitals, and after further applications he managed to get statistics from four hundred and thirty-nine hospitals.

The details obtained were tabulated for each year separately, and included the following points: the average number of beds occupied, or the average daily population of the hospitals; the total number of in-patients admitted; the average residence in days, and the number of deaths; thus:

No.	Name of hospital.	Years for which return is made.	Full No. of beds.	Average beds occupied.	Average No. of in-patients.	Average No. of patients in each bed.	Mean residence.	Mortality per cent.		District mortality per 1000.	Ratio of hosp. to district mortality.	Remarks.
								Beds.	Patients.			
1.	St. Bartholomew's	1861–64	676	547	5489·5	9·86	37·00	108·59	10·8			
2.	Guy's	1861–70	600	497·5	4913·6	9·87	36·99	98·15	9·9			

After giving a long list of tables, Tait says that he may fairly claim that the returns "now before us afford us a reasonable

basis for estimating what really is the mortality of our general hospitals."

He then proceeds to examine the statistics of various hospitals, and he does not hesitate to point out that owing to the very high rate of mortality in some of the well-known institutions, that special inquiry should be undertaken to endeavour to ascertain the cause of the mortality. Thus he says:

> It will be borne in mind that a few years ago, the lying-in department of King's College Hospital had to be closed on account of the terrible mortality among the parturient women, due to causes intrinsic to the hospital. Are its managers in a position to show that its high mortality—12·05 per cent., the highest in London except St. Thomas's, and quite as high as many fever hospitals—is not in any way due to the same causes as those which lead to the outbreaks of puerperal fever?

Tait asked the question, if the mere size of a hospital is a constant influence in raising the death-rate of a hospital? but he answers this in the negative, otherwise such a hospital as Guy's should have a greater mortality than University College, whilst in reality it is 2·41 per cent. less.

He inquires into the statement that has been made that the excessive hospital mortality in London was due to street accidents. But if the reports of the London hospitals be compared with such hospitals as the Leeds Infirmary, it will be seen that accidents in London are mere trifles compared to the accidents that occur in a purely manufacturing town, and that the entire mortality of the Leeds Infirmary is 3·101 per cent. less than the average of all London hospitals.

Tait enters into many questions which influence mortality in the hospitals, and makes some excellent remarks on the evil effects of crowding human beings together, and urges that one of the most important services to which the Government could direct the machinery of a scientific commission would be the inquiry into the influences of varying amounts of floor area, and cubic space, in hospitals.

We need not follow the author through the whole of his "Essay," but we may turn to the latter portion of it where he says that there are other means of testing the results of hospital

practice, as by taking groups of cases which are strictly comparable, such as ovariotomies and amputations: the latter had been selected by several writers on this subject, but more especially by Simpson. Tait had gone to great trouble to collect a large mass of statistical information of this kind, and this he has tabulated in an appendix to his essay. The conclusion that he arrived at with regard to amputation mortality was: "that the whole of my statistics tend to prove that after the number of beds in a hospital exceeds 100, the risk to life becomes so much increased that it is questionable whether any hospital should be of larger size than this. If circumstances make it necessary that the hospitals should be larger, most undoubtedly special arrangements and precautions should be taken to obviate the extra risk which is involved."

Tait then considered the subject of ovariotomy, and at the outset says: "that the first surgeons who attempted ovariotomy, and failed to make it a success, were surgeons who performed the operation in large public hospitals, and it was not until the operation was done in small private hospitals that it became less fatal."

He said that he had collected 271 cases of ovariotomy performed in hospitals having more than 100 beds. Of these 58·1 per cent. died. "These figures have convinced me that this operation should not be performed in a hospital, in the ordinary sense of the term, of any kind whatever; and I think that the most enthusiastic conservative will hardly dare venture to support its performance in large hospitals." [1]

He brings forward the good results of Thomas Keith, of Edinburgh, to further support his arguments.

Tait's essay was received with anything but approbation. Reed says: "No work of equal size ever produced a more pronounced impression on the profession of England." No

[1] "As it was supposed that the infected air of the hospitals might be the cause of the invariably fatal results of that operation [ovariotomy], the Assistance Publique hired an isolated house in the Avenue de Meudin, near Paris, a salubrious spot. In 1863, ten women in succession were sent to that house; the neighbouring inhabitants watched those ten patients entering the house, and a short time afterwards saw ten coffins being taken away. In their terrified ignorance they called that house the 'House of Crime.'"—Réné Vallery-Radot, "The Life of Pasteur," vol. ii. p. 16.

sooner had it been published than a newspaper war began. This was carried on in the *Lancet*, and other papers, and Tait had to reply to criticisms from all sides. Had he sought notoriety, he could not have achieved his end more quickly. Naturally the men attached to the London hospitals were up in arms to defend, or make excuses for, their institutions, whose high rate of mortality he had been very careful to expose in his "Essay."

Some of the critics proceeded to point out that Tait had announced that the essay was based on statistics of the hospitals of Great Britain for fifteen years, while in the preface he says:

> "In 1871, I made an attempt to collect the details given in my tables from every hospital in Great Britain for the preceding decade; but it will be seen that the results attained were such as could hardly be depended upon for accuracy, and in some respects they were so meagre as hardly to be worth the trouble involved in collecting them; I do not, therefore, place great reliance on them for my conclusions." Later he goes on to say: "The enormous mass of figures employed, including over two millions of patients for the two periods, of which more than three-quarters of a million belong to the six years from 1870–75 inclusive. . . ."

These statements were immediately seized upon by Tait's opponents, who proceeded to point out that, on his own showing, the figures referring to a million and a quarter of the patients out of the two millions came under the first decade, and "the results obtained were such as could hardly be depended upon for accuracy." The critics said, therefore, that they must be excused from accepting deductions arrived at by the author from figures, when two-thirds of the facts are admitted by the author to be founded on figures that could not be "depended upon for acccuracy."

The *Lancet* and other papers held that it was impossible to get any valuable results from the comparative percentage mortality in general hospitals. The *Lancet* further stated that it doubted the accuracy of Tait's figures; while Greenfield, in a letter to that paper, alluded to Tait's results as masses of "crude figures."

It would be difficult to say at this distance of time, whether

Tait's essay did any real good. It was all very well for him to point out that the mortality in certain hospitals was excessive, but he did not, and could not, point out the remedy. He said that in an inquiry into hospital mortality there were three steps in the process, and that the second step was to inquire into the causes of the fluctuation of hospital mortality, and the third step was to discover remedies for the defects laid bare. The first step he dealt with at length, " but the second step I can deal with only partially —it is so wide a subject. Upon the third, I could not touch without much more extensive data than I have yet in my possession," and in these statements Tait really admits that he did not know what was going on in Scotland and in Europe; in short, Tait had entirely missed the work that Lister had been engaged on for years. If Tait had only made himself acquainted with Lister's paper, " On the Effects of the Antiseptic System of Treatment upon the Salubrity of a Surgical Hospital," [1] or the later paper, " An Address on the Effect of the Antiseptic Treatment upon the General Salubrity of Surgical Hospitals," [2] then he would not have taken the trouble to write his essay on hospital mortality; or, if he had, he would have been able to write on the third step and discover remedies " for the defects laid bare."

The first of Lister's papers alluded to above has now become historical, and as that paper is one of the sign-posts in the history of the evolution of modern surgery, and as it gives the solution of the problem that was troubling Tait, we propose to give an extract from it.

When Lister left Glasgow for Edinburgh he gave his experiences of the condition in which he found the wards in the Glasgow Royal Infirmary, and the condition in which he left them. After giving a description of the wards as he found them he says:

> But to the great disappointment of all concerned, the noble structure proved extremely unhealthy. Pyæmia,

[1] The *Lancet*, 1870, vol. i. p. 4.
[2] *Brit. Med. Jour.*, 1875, vol. ii. p. 769.

erysipelas and hospital gangrene soon showed themselves, affecting on the average most severely those parts of the building nearest to the ground, including my male accident ward.

Lister then goes on to state that, at that time, the death rate was so great, it was necessary to close some of the wards. Then he continues :

A crisis of this kind occurred rather more than two years ago in the other male accident ward on the ground floor, separated from mine merely by a passage twelve feet broad, where the mortality became so excessive as to lead not only to the closing of the ward, but to an investigation as to the causes of the evil, which was presumed to be some foul drain. An excavation made with this view, disclosed a state of things which seems to explain sufficiently the unhealthiness that had so long remained a mystery. A few inches below the surface of the ground, on a level with the floors of the two male accident wards, with only the basement area four feet wide intervening, was found the uppermost tier of a multitude of coffins, which had been placed there at the time of the cholera epidemic of 1849, the corpses having undergone so little change in the interval that the clothes they had had on at the time of their hurried burial were plainly distinguishable. The wonder now was, not that these wards upon the ground floor had been unhealthy, but that they had not been absolutely pestilential. Yet, at the very time when this shocking disclosure was being made, I was able to state in an address, which I delivered to the meeting of the British Medical Association in Dublin, that during the previous nine months, in which the antiseptic had been fairly in operation in my ward, not a single case of pyæmia, erysipelas, or hospital gangrene had occurred in them, and these, be it remembered, not only in the presence of conditions likely to be pernicious, but at a time when the unhealthiness of other parts of the same building was attracting the serious and anxious attention of the managers. Supposing it justifiable to institute experiments on such a subject it would hardly be possible to devise one more conclusive.

Lister brings his paper to an end in the following words :—

It is obvious that the facts recorded in this paper are of extreme importance with reference to the vexed question of hospital construction. With the view of assimilating the atmospherical conditions of our large hospitals to that of a private dwelling, it has been lately proposed to do away with them altogether in their present form, and to substitute

> for them congeries of cast-iron cottages, capable of being occasionally pulled down, cleansed, and reconstructed—a plan which, besides involving enormous expense, would interfere most seriously with efficient supervision of the patient, and with the teaching of students at the bedside. But from what has been related above, it is plain that no natural alteration of the existing system will be required. We have seen that a degree of salubrity, equal to that of the best private house, has been obtained in peculiarly unhealthy wards of a very large hospital, by simply enforcing strict attention to the antiseptic principle. And considering the circumstances of these wards, it seems hardly too much to expect that the same beneficent change, which has passed over them, will take place in all surgical hospitals, when the principles shall be similarly recognised and acted on by the profession generally. The antiseptic system is continually attracting more and more attention in various parts of the world; and whether in the form which it has now reached, or in some other and more perfect shape, its universal adoption can be only a question of time. The noble institutions of which our country is justly proud, admirably adapted alike for the treatment of the sick and instruction of the students, will then be cleared of the only blot that now attaches to them —the malignant influence of impure atmosphere.

These prophetic words of Lister, if they had been read by Tait had made no impression on him. But they had made such an impression on the profession on the Continent, that when, in 1875, Lister visited many of the continental clinics, he saw that the predictions made in the concluding paragraphs of his paper, quoted above, had been realised, and he was able to say in his second paper entitled, "An Address on the Effect of the Antiseptic Treatment upon the General Salubrity of Surgical Hospitals," "that prediction, I think I may say, is now in course of fulfilment."

Lister during his tour on the Continent, went to Copenhagen, Munich, Leipsic, Halle, Berlin, Magdeburg, and Bonn, and in all these places the antiseptic principle had been adopted, and he heard from every hospital that he visited that pyæmia, erysipelas, and hospital gangrene had ceased to exist.

Thus we see that while the surgeons in London, and the provinces, were crying out about their excessive hospital mortality, those in Glasgow and Edinburgh and on the

Continent who had followed the principles laid down by Lister had already solved the problem. Yet the very last person in the profession who would have accepted Lister's solution of the problem would have been Tait himself. In later years, when Lister came to live in London and demonstrated the truths and expounded the principles of his teaching, these were ridiculed by the then President of the Royal College of Surgeons of England, Mr. [afterwards Sir] William Savory, and they were reviled by Tait, who said: "Lister's antiseptic doctrines and practice were based upon a conception altogether false from my point of view. . . . Listerism of twenty, fifteen, twelve, and ten years ago, is dead . . . the doctrines and practice of Lister had been the basis of the strongest surgical craze of the nineteenth century."

We shall return to this subject in a later chapter, when we come to deal with Tait's attitude to Listerism.

Quite early in his career Tait saw how advantageous it would be to have his patients housed in private houses, or in small special hospitals. But the special hospital was a thing almost unknown in England in 1870, and Godlee says, in speaking of Lister's work in London, that in 1877 there were only one or two such houses in all London. The idea was new, and it was long before the public recognised the advantage of nursing homes, and yet longer before the family doctor ceased to complain that in nursing homes patients were removed from their observations at the most interesting and critical stage of the disease. Godlee remarks that it must be owned that the earlier homes were far from perfect, and these were followed by smaller and less well-managed institutions, which justified the objections of the public and the general practitioners.[1]

Tait recognised that abdominal work was not a success when carried out in large general hospitals, and entered, as we have seen, into the scheme for establishing at Birmingham the first special hospital in England for the treatment of the diseases of women.

Long before the special hospital became common in England it existed and flourished in America.

[1] Godlee, "Lord Lister," p. 408.

Joseph Price tells us that McDowell made a private hospital of his house, and " that it was precisely the early work of Sims with his private hospital, a little chicken-house, or wooden shed, or waggon-shop, that enabled him to do so much, and he made these successful efforts after the profession had deserted him."

Battey also established a private hospital and educated the members of the profession. Marion Sims came to settle in New York, and he has left us a most interesting account of how he established, in 1855, the State Hospital for Women, which soon became the centre of the best gynæcological work of those times. For " the new speciality," as Howard Kelly remarks, " the first of all modern specialities, was the child of a new spirit in a new age, born in an era of healthy scepticism, and fostered by every new and quickening influence in an age pre-eminent in scientific investigation and progress."

After the special hospital became established at Birmingham, and became the centre for the " Birmingham School," other provincial towns established special homes where operations could be performed, and London followed suit. It is no exaggeration to say that the needs of gynæcology and abdominal surgery led to the establishment of special hospitals in England, and these were made possible by the reforms that took place in the large general hospitals, which gave us, through the work of Florence Nightingale and her training school, nurses fit to carry out the details of the antiseptic technique of Lister, and later on the aseptic technique of Lawson Tait.

X

Treatment of Chronic Inversion of the Uterus

1878

At the May meeting of the Obstetrical Society, Tait read a communication on "Treatment of Chronic Inversion of the Uterus." He had already had some experience with this condition, for in 1869, acting on the suggestion of Robert Barnes,[1] he had succeeded in reducing an inversion, but unfortunately a fatal result followed.[2]

Observations on chronic inversion of the uterus date from Soranus and extend up to the present time, when abdominal surgery has been called to the aid of the gynæcologist in dealing with some of the cases that have not been successfully treated by the repositor.

Innumerable suggestions have been put forth at various periods for the cure of this condition, and as the plan suggested by Tait has proved to be very effective, we propose to mention some of the schemes suggested before his time, in order to show more clearly how his practical brain gave to us a plan as efficient as it is simple.

Some writers, one of the most recent being Crossen,[3] have maintained that Hippocrates wrote clearly on the subject of inversion of the uterus. We are not disposed to admit this; it is quite true that the Father of Medicine wrote concerning prolapsus of the uterus, and alluded to a case of complete procidentia when he remarked that in some cases "the uterus hangs down like a scrotum." But this was not a case of inversion; it was a case of complete prolapsus.

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[1] Barnes, "Diseases of Women," London, 1873, p. 728.
[2] *Trans. Obstet. Soc.*, London, vol. xi. p. 174, 1869.
[3] *Jour. Amer. Med. Assoc.*, April 4, 1914.

Soranus, however, does mention inversion, and remarks that traction on the cord may produce it. Galen knew that it could be caused by spontaneous abdominal pressure, as a passage in his works clearly shows.

Soranus and Aetius both refer to vaginal hysterectomy in cases of prolapsed wombs, which either could not be restored, or which had become gangrenous, and they state that the patients had lived to a good old age after some of these operations.[1] We admit that it is quite possible that some of the cases operated on were cases of chronic inversion.

Thomas says that it is not certainly known whether the condition of inversion of the uterus was properly understood before the time of Ambrose Paré. Since his epoch it has been fully described by his successors, and all its pathological features, its various symptoms, and its manifold dangers have been thoroughly appreciated.[2]

From the time of Paré down to the beginning of the eighteenth century, very little was attained in the way of treatment. It was known that it was possible to replace by taxis a uterus recently inverted, but for those cases that had been inverted for months, or years, no resource existed except amputation. Blundell, in 1822, says that he knew of four cases in which the ligature had been applied . . . adopting the same procedure as that used in extirpation of a large polypus . . . for extirpation of the uterus in chronic inversion, and all four cases did well.

The first cases of chronic inversion which were successfully reduced by taxis were mentioned by Daillez, in a *thesis* that appeared in 1803. He says that Labarre de Benzeville had effected the reduction as late as the eighth month, while Baudelocque had reduced an inversion which had existed for eight years!

In 1847, M. Valentin was successful by introducing one hand into the vagina and making counter pressure by the other over the abdomen.

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[1] Stewart McKay, "History of Ancient Gynæcology," London, 1901, p. 295.

[2] Thomas, "Diseases of Women," London, 1891, p. 451.

In 1852, Canney effected reduction in a case of five months' standing, and in the same year M. Barrier was successful in a case that had existed fifteen months.

Up to the year 1858, reposition of inverted uteri may be said to have been limited to a replacement effected within short periods after parturition. About this period Tyler Smith of London, in a case of inversion of twelve years' standing, effected the reduction by maintaining pressure upon the tumour, and this in turn on the os by an air pessary during several days. The case was treated in 1856, but he did not report the case to the Royal Medical and Chirurgical Society until April 13, 1858.

In the meantime, in February, 1856, and again on March 12, 1858, Professor James White, of Buffalo, was able to replace two cases of chronic inversion by pressing a cup repositor against the fundus, and then pressing his chest against a spiral spring attached to the repositor. White reported his cases to the Buffalo Medical Association in April, 1858, and they were published in the *American Journal of the Medical Sciences*, July, 1858.

With the publication of the cases of Smith and White, we may date the beginning of the modern treatment of chronic inversion by pressure and taxis, and after their cases were published many old standing inversions were treated either by taxis alone, or taxis combined with pressure by bags of air or water placed in the vagina, and Noeggerath, Teale, West, Bockenthal, Hakes, and Schroeder all reported cases where inversion had been reduced in spite of it having existed from one to fifteen years.

The methods of White and Smith were essentially different, though alike in that they secured uniform elastic and sustained pressure.

Previous to the discovery of White, Viardel, in 1674, had published a description of a rod with rounded ends, which at a later date Chailley Honori bent to conform to the pelvic curve, and with this rod pressed up against the inverted fundus, they sought to replace the organ. Then Madame Boivin suggested rods with cupped ends, and von Siebold introduced a repositor which consisted of a curved stem

surmounted with a sponge. Later Barnes and Braxton Hicks modified the repositor of von Siebold; the former attaching a hollow rubber cup to the curved stem, while Hicks covered the end of his instrument with india-rubber.

Thus we see that before Tait's paper, which we shall now deal with, the progress of discovery advanced from the straight stem of Viardel, to the rod of Chailley Honori with the pelvic curve; from the rod with the rounded end to the rod with the cup-shaped end.

Tait opens his paper by alluding to the proposition of Simpson and Tyler Smith to rectify the inversion by continuous pressure. He then mentioned the instrument devised by White of Buffalo. He said that he considered that no plan could so efficiently diminish the size of the inverted uterus as pressure upon it by a conical cup, and that it was equally evident, that the best way of dilating a contracted and inverted cervix was to make it do the work, as it were, by pressing upon itself. He, therefore, abandoned the pelvic curve of the oval instrument as useless, had box-wood cups made of three different sizes, each with a straight stem about six inches long, and with notches at the end for strings.

The case to which the treatment was applied was that of a young woman, aged twenty, who had been delivered by a midwife ten weeks previously. The uterus was completely inverted, and thorough involution had taken place, so that the inversion had passed into the chronic stage. After she had been in hospital for six weeks, the large rigid cup was introduced under ether, and a double thread of elastic was applied to the stem, and fastened to the waist in such a way as to give a strain probably not exceeding a pound at the utmost. The next morning the uterus was found reinverted and enclosing the cup. On the removal of the latter it was found that the reinversion was not complete, and the smallest sized cup was therefore introduced, with the effect of securing perfect reduction.

Tait concluded his paper with the remark that the simplicity of the treatment in this case, he thought, entitled him

to say that, in future, very few cases should be submitted to the extreme measure of amputation.[1]

Now in recent works which deal with the treatment of chronic inversion, we find that White's method and Aveling's method are always mentioned, but Tait's method is apparently forgotten. This is not just to Tait. The principle of action in the methods of White, Tait, and Aveling is identical, although the manner of evolving the force differs. In White's method the elastic pressure is derived from a steel spring and directed, through an inflexible rod "curved to conform with the curve of the vagina," against the inverted fundus; while in Tait's and Aveling's method the elastic force is derived from elastic bands and exerted upon the fundus, through a straight rod in one case, and a sigmoid rod in the other, in the direction of the axis of the superior strait of the pelvis.[2]

Now we wish to show that Tait has a prior claim to Aveling in this matter.

Tait read his communication at a meeting of the Obstetrical Society of London, held on May 1, 1878; Dr. James H. Aveling, Vice-President, being in the chair. The report of the meeting was published in the *Lancet*, June 1, 1878. At this meeting, when Tait had finished reading his paper, "Dr. Aveling remarked, that the case would serve to encourage us in the treatment of a very difficult condition, which till recently had been thought only relievable by amputation. The use of straight stems was, however, not a novelty; they had been first advocated by Madame Boivin, and Dr. Braxton Hicks had employed an instrument very similar to a stethoscope."

Aveling did not make any remarks about any instrument that he had invented, but at a meeting of the Society held on June 5, Aveling "exhibited some repositors which he had devised for the treatment of inversion of the uterus, and related the particulars of a very obstinate case, which he had recently succeeded in reducing by their agency. His repositor consisted of cups of various sizes attached to stems,

[1] The *Lancet*, June 1, 1878.
[2] Busey, "System of Gynæcology," Mann, vol. ii. p. 712.

which differed from those of all other repositors in having both a pelvic and a perineal curve." [1]

Thus we see that Tait made his announcement on May 1, while Aveling did not make his until June 5. When did Aveling first apply his apparatus? We have discovered the date, after much hunting, in a paper contributed by him to the *British Medical Journal*.[2] In this paper Aveling says:

> Mrs. W. came under my care, suffering from chronic inversion of the uterus. She had been treated by several eminent gynæcologists, with a variety of repositors, and by taxis, and had been sent to Sir Spencer Wells to have the uterus removed. The patient, however, thought she would have one more attempt at being cured without mutilation and fell into my hands. It was for this case my repositor was made, and it was applied, without causing much pain, on May 7, 1878, at 3 p.m. The cup was fitted to the fundus; and by means of four elastic rings, two in front and two behind, all being fastened to a belt kept up by braces, pressure was exerted. . . .

Here, then, we discover the interesting fact that Aveling invented his repositor, and applied it *seven days after he had heard Tait read his paper at the Obstetrical Society, May* 1, 1878.

In comparing Tait's apparatus and Aveling's, the essential difference between the two is that in Tait's apparatus the stem of the repositor is straight, while in Aveling's the rod is a sigmoid. Aveling, in the lecture referred to above, says: "The straight cupped repositor just escapes being a perfect instrument. It must always act to a certain intent indirectly, causing the cup to slip back past the fundus, or to urge it against the posterior wall of the vagina in a line diverging 25° from the axis of the pelvic inlet." He claims with his sigmoid repositor that he can press the uterus in the direction of the upper axis of the pelvis, thereby rendering the reducing of an inverted uterus more easy and certain. Aveling is very careful to avoid all mention of Tait's apparatus, though he refers to the methods of several other

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[1] The *Lancet*, June 22, 1878.

[2] "A Lecture on Inversion of the Uterus," *Brit. Med. Jour.*, March 13, 1886.

operators, and says: "It is due to Dr. R. Barnes to state that he was the first to apply elastic pressure by india-rubber bands to the stem of the repositor." [1]

If, then, we sum up the claims of the various surgeons who have contributed something towards the evolution of a repositor which acts on the inverted fundus by continuous pressure, we find that in Dujès' book on "Diseases of Women," by Madame Boivin and Dujès, that the proposal to deal with chronic inversion of the uterus by means of continuous gentle pressure is suggested in the following words: "Might not pressure from below upwards be also made available for reduction of the inverted uterus? A pessary, like that of a cup and ball, might for this purpose be introduced in the vagina." Sir James Simpson also recommended a similar procedure. Then we have the method of elastic pressure by a pessary in the vagina, retained by a bandage, first introduced by Tyler Smith. Then we have the method of Professor White of Buffalo, whose repositor, cup-shaped, was pressed up by a spring placed against the surgeon's chest. Then we have Robert Barnes introducing his curved stem, which exerted pressure by means of elastic bands; and lastly we have Tait's cup repositor with a straight stem, and Aveling's repositor with a sigmoid stem, both exerting pressure by elastic bands.

Tait continued to employ his own method, and in his later work on the "Diseases of Women" (1889), he states that "during the last ten years I have had thirteen cases of chronic inversion of the uterus, and have succeeded in reducing them all but two. In one the patient would not submit, and in the other I amputated the inverted organ by mistake."

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[1] In Robert Barnes' "Clinical History of Diseases of Women" (1873), we find this: "In my second case I used the elastic pessary. . . . At the end of the stem, which is outside the vulva, are attached strong rubber bands, which are brought up before and behind to be attached to the abdominal belt."

XI

Cholecystotomy

1879

Christopher Martin, the well-known surgeon of Birmingham, who for many years was Tait's assistant and his intimate friend, has written thus about the year 1879 :

> The next year, 1879, was in my opinion his *annus mirabilis*. He performed his first cholecystotomy, removed his first pyosalpinx and hydrosalpinx, described his flap-splitting operation for repair of the perineum, and introduced his method of reposition of the inverted uterus, and also of dilating the cervix by means of continuous elastic pressure.[1]

A truly wonderful year, but as we have shown above, Tait did not introduce his method of reposition of the inverted uterus this year, but in 1878 ; on this point Martin is in error.

Of the operations mentioned above, we shall first deal with the evolution of our knowledge of affections of the gall-bladder ; also with the surgery of the gall-bladder, with special reference to Tait's operation for gall-stones, so as to appreciate in full what he did for gall-bladder surgery.

Tait himself was under the impression that he was the first surgeon who had performed the operation of cholecystotomy *successfully*, for he says in a paper on " The Surgical Treatment of Gall-stones," [2] " The first solitary kind of gall-stones was very well illustrated by the first case of cholecystotomy which I performed ; the first successful case on record." His statement would have been correct if he had said, " the first successful case in England," but we shall show that his statement as it stands is not correct.

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[1] *Brit. Med. Jour.*, June 24, 1890.
[2] The *Lancet*, August 29, 1885.

When we consider that since autopsies have been systematically carried out in the hospitals of all large cities, gall-stones have been found in from 5 to 10 per cent. of all the bodies examined, and that the figures increase very markedly the older the subject is, we are struck by the fact that there is little mention of gall-stones to be found among the medical authors of antiquity, who were renowned for the accuracy of their observations. Thus it is not until we come to Alexander Trallianus—one of the most eminent of the ancient physicians, who, according to Greenhill,[1] "may safely be put in the sixth century after Christ"—that we find gall-stones mentioned. He remarks that in cases of obstruction of the liver, the juices being overheated become like stones.[2] Later on, during the tenth century (A.D.), Haly Abbas, the great Persian physician, makes it plain that the ancients were acquainted with hepatic calculi, or gall-stones, for he says, "Some relate that calculi are formed in the liver, cæcum, and colon."

In view of the fact that the old Greek physicians were good clinical observers, and at the same time thorough students of anatomy, we are forced to the conclusion that gall-stones were less frequent in those days than they are in modern times. An explanation for this difference might be sought in the mode of life: their food was different and prepared in a different way, and physical exercises were an important factor during the years of adolescence. As the men grew older they still continued to indulge in much and varied physical exertion; and lastly, the clothing of the women did not cause anything that is analagous to our corset liver.[3]

Gentilis of Foligno,[4] according to Donatus, is said to have found, in the fourteenth century, stones in the gall-bladder and the cystic duct of a corpse that he was embalming.

Gall-stones are first mentioned by Antonius Benivenius,

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[1] Smith, "Dictionary of Greek and Roman Biography and Mythology," London, 1853, vol. i. p. 126.

[2] Francis Adams, "The Seven Books of Paulus Aegineta," London, 1844, vol. i. p. 566.

[3] Hoppe-Seyler, "Diseases of the Liver, Nothnagel's Practice," p. 525.

[4] Gentile de Foligno was a victim of the Black Death in 1548.

who died in 1502.[1] These were found in a woman who had suffered with pain in the abdomen, and after death an examination showed stones not only in the gall-bladder but in a sacculated cavity on the surface of the liver.

Dezeimeris, in his notice of the life of Benivieni, says: "il est aussi le premier, avec Vésale et Fallope, qui ait examiné et décrit les calculs biliaires." [2]

After this we find that Fernelius (1554), Colombus (1557), Camenicenus (1651), and Paracelsus (1563), all recognised the existence of gall-stones. In 1570 Ferrandus published a case of rupture of the gall-bladder, caused by a large gall-stone. Later Glisson's examinations (1654) were of great importance in the further development of our knowledge of gall-stones. He described his own case and gave a good picture of an attack of hepatic colic combined with icterus, and the peculiar pain of this condition radiating into the region of the clavicle.

Blasius (1677) was the first to give a description of the formation of abscesses as the result of gall-stones.

In 1618, according to Thudichum, Johannes Fabricius is said to have removed gall-stones from the gall-bladder of a living subject. Fabricius Hildamus refers to the operation, but it is not clear that the operation was not done post-mortem; the only evidence of the subject being alive is the somewhat dubious expression "delineatio horum lapidum ad vivum facta." [3]

In 1630 Zambeccari, an Italian, performed cholecystotomy on a dog; the animal recovered and two months later was killed, when it was found that the omentum and bowels were adherent over the stump of the cystic duct.

Ettmüller (1708), in a dissertation entitled "De ictero flavo, nigro et albo," made known the fact that a student Teckof, in Leyden, removed gall-bladders from several dogs without detriment to life or health.

Stalpart van der Wiell (1687), Thilesius, Amyard (1738),

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[1] Hoppe-Seyler is in error when he states that Benivenius died in 1592.

[2] "Dictionnaire Historique de la Médecine Ancienne et Moderne," Paris, 1828.

[3] Greig Smith, "Abdominal Surgery," London, 1896, vol. ii. p. 994.

and Müller (1742) have described the removal of gall-stones which were evacuated on opening an abscess; these operations were the result of accident, not deliberately planned operations on the gall-bladder.

To Petit [1] is undoubtedly due the merit of having founded the surgery of the gall-bladder. Petit (1674–1750) was the leading French surgeon of the early part of the eighteenth century. He was the inventor of the screw-tourniquet, gave the first account of the formation of clots in wounded arteries, and was the first to open the mastoid process.

Petit, in 1733, mooted his project of attacking the gall-bladder, and he discussed with great acumen the diagnosis of tumour of the gall-bladder, and the sequence of biliary retention.

One year after Gottfried Müller had enlarged the opening of a fistula, which resulted from an abscess which had burrowed through the stomach and the abdominal walls, and had removed a large stone from the gall-bladder, Petit called attention to this form of suppuration of the gall-bladder, due to the presence of gall-stones, and emphasised the necessity of drawing the pus away.

He only operated, however, in cases when adhesions were present between the gall-bladder and the peritoneum, as shown by immobility of the gall-bladder and inflammation of the skin. He advised puncture, the finding of the stone with a probe, enlargement of the fistula, and extraction of the stone. In one case, the patient being a woman, he operated with success at several sittings, and he remarks: "How many people have died because the disease was not recognised, or because no operator could be found who would undertake to rid them of the disease by means of an operation." [2]

English surgeons, with the exception of Samuel Sharp, ignored Petit's work, but on the Continent Morand (1757), Haller (1760), Herlin (1767), and Bloch of Berlin, by observations and experiments, sought to advance Petit's labours:

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[1] *Mem. de l'Acad. Roy. de Chir.*, t. 1, p. 163, Paris, 1743.
[2] Moynihan, "Gall-stones and their Surgical Treatment," 1905, p. 294.

thus Bloch, in 1774, attempted to create an artificial adhesion of the gall-bladder to the parietal peritoneum by means of the application of irritant materials to the skin, and in three cases he was able to operate successfully.

Bromfield, in 1773, wrote to condemn the operation.

Morgagni (1767) drew a most lucid picture of the state of knowledge of gall-stones of that day, in his celebrated work, "De sedibus et causibus morborum"; while Chopat, Desault, and Walter (1796), in the second part of the eighteenth century, all contributed towards a better understanding of the subject of cholelithiasis.

Richter, the famous German surgeon, appeared in 1798 with a litho-triptor, and explained how fragments could be removed by washing; he first suggested that adhesion of the gall-bladder to the parietal peritoneum was not an essential preliminary to an operation. He wrote:

> Is then an escape of bile into the belly cavity to be feared when the gall-bladder is not adhered to the peritoneum if the trocar be used, and be left in the wound after the gall-bladder is empty? Have we not cause to hope that the cannula will cause the gall-bladder to adhere to the peritoneum, preventing it from moving away by the creation of adhesions?

In the first half of the nineteenth century, as Greig Smith has pointed out, little advance was made towards cholecystotomy. Delpech, in 1816, rather decried the operation, and Good, in 1825, spoke of it as being of doubtful value. In 1828, Sebastian described an operation in which the wound was incised down to the peritoneum and adhesion was secured between the gall-bladder and the peritoneum before opening the tumour—a suggestion afterwards modified by Graves of Dublin. In 1847 Dufresne recommended opening by caustic; and soon after Récamier advised the trocar.

We now come to Thudichum, who in 1859 suggested that the operation of cholecystotomy should be performed in two stages, the gall-bladder being stitched to the abdominal wound in the first stage; and in the second, the gall-bladder should be opened after several days. But his views found

no favour, though Hilton thought that cases might be found suitable for operation.[1]

In the year 1867 Bobbs of Indianapolis was the first to perform the modern operation of cholecystotomy. Bobbs was the Professor of Surgery at the Medical College of Indiana, and is said to have been a bold and original operator, who, amongst other things, had operated successfully on a case of extra-uterine pregnancy.[2]

As the case recorded by Bobbs [3] is the initial step in the modern surgery of the gall-bladder, we shall give it in brief.

The patient was a woman, aged thirty years. The increase in size of the gall-bladder had been gradual for about four years; but the true nature of the enlargement was in doubt prior to the operation. On June 15, 1867, Bobbs made an exploratory incision extending from the umbilicus to the pubis. This revealed extensive adhesions of the omentum for an inch above the umbilicus. Tearing through the adhesions Bobbs reached a sac about five inches long. As no pedicle could be discovered the lower part of the sac was incised, "when a perfectly limpid fluid escaped, propelling with considerable force several solid bodies about the size of ordinary rifle bullets."

The gall-bladder was then emptied, the incision in its walls stitched, and the ends cut closely and returned into the abdominal cavity.[4] The external wound was closed, and the patient made a rapid recovery.

Though this was not a *premeditated* cholecystotomy, it was a brilliant piece of work and speaks volumes for the skill and the cleanliness of the surgeon.

The operation performed by Bobbs, however, did not attract any attention, probably on account of being published in a journal that was unknown in Europe; consequently we find, when Daly read a paper before the Clinical

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[1] The *Lancet*, vol. xxii. 1859.

[2] Kelly, "A Cyclopedia of American Medical Biography," 1912, vol. i. p. 92.

[3] *Trans. Indiana State Med. Soc.*, 1868, p. 68.

[4] Moynihan (*loc. cit.*) says in his account of this operation: "After opening the abdomen the gall-bladder was brought up into the abdominal wound, opened and emptied, and sutured to the parietal peritoneum."

Society of London,[1] "On a Fatal Case of Gall-stones," that the report stated the case was of interest from the fact that surgical operation for removal of the gall-stone was suggested during life. The patient, who was a woman of intelligence, had herself expressed a desire to consult a surgeon to see if there was any possibility of removing the gall-stones by operation. Mr. Maunder was consulted, but would not operate, and later on he made the following remarks :

> By experiments on the dead subject he found it possible to pick up the peritoneal coat alone of the gall-bladder, and stitch it to the abdominal wall. Provided, then, that on seeing the patient he could feel the gall-bladder, he was prepared to repeat the operation on the living subject. He proposed having fastened the gall-bladder to the wound, to leave it for three days so that adhesions might take place before opening it, in order to prevent the passage of bile into the peritoneal cavity.

Still later, Handfield Jones [2] proposed that in a case when the stone was in the duct an endeavour should be made to push it into the duodenum after the abdomen had been opened.

Kocher [3] takes a share in the honour of developing gall-bladder surgery, for, in 1878, he opened the abdomen and packed round the gall-bladder with Lister's gauze, and six days later, when adhesion had formed, he opened the gall-bladder and emptied it ; the patient recovered.

We now come to deal with the three operations which were practically the beginning of modern gall-bladder surgery.

The first case was operated on by Keen.[4] This case ended in death. There was found a distended gall-bladder ; the condition of the ductus choledochus, however, does not appear in the report of the post-mortem examination ; that might have enlightened us as to the cause of the distended bladder.

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[1] The *Lancet*, November 4, 1876.

[2] *Medical Times and Gazette*, 1878, 1, 246.

[3] Moynihan says : " The credit of performing the first intentional and successful cholecystotomy, in two stages, belongs to König, who operated in 1882." Kocher had, as shown above, done this in 1878.

[4] *Amer. Jour. Med. Sciences*, January, 1879, Keen ; also paper by Musser and Keen, October, 1884.

The second case is the well-known one operated on by Marion Sims, and though unsuccessful the operation had an important influence in advancing the surgery of the gall-bladder.

The patient was a woman, aged forty-five. She complained of a pain in the right lumbar region, and later on in the right hypochondrium, and there a tumour could be felt which grew larger day by day. The patient vomited constantly, had jaundice, passed clay-coloured stools, sometimes bloody, always offensive.

Sims saw the case in Paris and advised aspiration. This was done, the fluid withdrawn, showing no bile.

The patient then grew worse, bled at the nose, had severe pain in the right side, and became so ill that Sims proposed to cut down on the tumour, freely open it, empty its content, and, if it proved to be the gall-bladder, to attach its incised border to the edges of the abdominal incision, and thus make a fistulous opening that would insure against its refilling. "We were encouraged to make a permanent fistulous opening because, first, we saw such improvements in all her symptoms follow the temporary removal of the fluid by aspiration; second, it would be in imitation of the efforts of nature in all similar cases when recovery has taken place." [1]

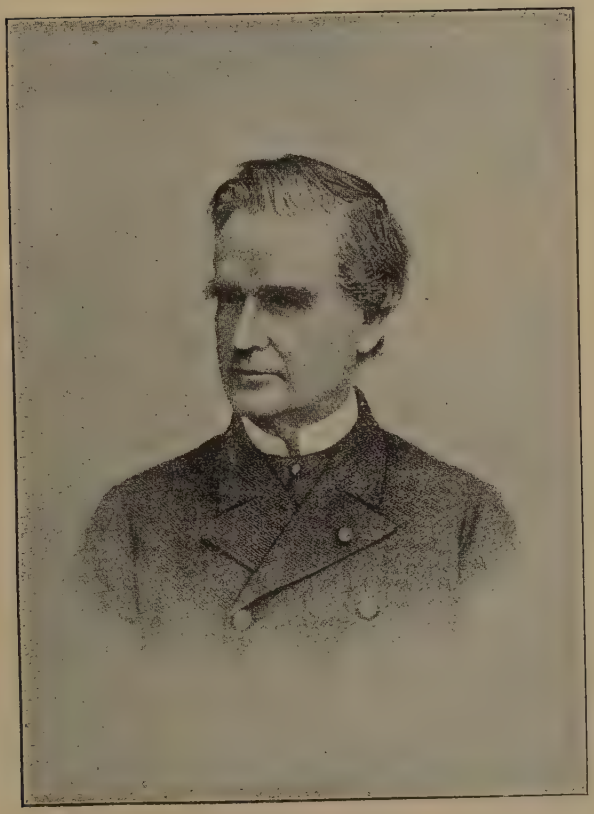
The operation was performed under antiseptic precautions and the carbolic spray. An incision to the right and one inch above and two below the umbilicus was made, the peritoneum was opened and the cyst aspirated, twenty-four ounces of dark fluid being drawn off. The gall-bladder was then incised and sixty stones removed, and as the bladder was eight inches long, two inches were cut off and "the puckered mouth of the amputated cyst was crowded into the upper angles of the abdominal incision and there secured with eight fine carbolised silk sutures."

The operation lasted an hour and sixteen minutes, and the patient lived eight days.

Death is said to have been from hæmorrhage, but after

[1] *Brit. Med. Jour.*, 1878, 1, 811.

PLATE IX.



MARION SIMS.

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To face page 112.

reading the whole case very carefully, we think that the black vomit indicated sepsis.

Sims claims that the operation was unique, and says that "as this is a new operation we must find a name first, and I think CHOLECYSTOTOMY (χολή, gall; κύστις, bladder; τομή, incision) will answer"; thus Sims gave us a term that is used and recognised everywhere, and it has served as a basis for the creating of several other terms used to express operations either on the gall-ducts or gall-bladder—such as CHOLECYSTECTOMY, CHOLECYSTENTEROSTOMY, CHOLEDOCHOTOMY, and several others which we shall allude to when we trace the further steps in the evolution of gall-bladder surgery.

We now come to Tait's first cholecystotomy, the first successful one performed in England. As early as 1869 Tait had removed gall-stones from a suppurating gall-bladder by slitting up a sinus, discharging fluid from the umbilicus.

In August, 1879, a woman aged forty was sent to Tait by Dr. Colles, of Bridgnorth, on account of an abdominal tumour. Her history was that in 1878 she had begun to suffer from severe spasmodic pains in the right side, and in September she noticed a swelling at the seat of the pain. The seat of the pain was over the right kidney where there was a heart-shaped tumour, hard to the touch, and when the patient was examined under ether, it was found to be so movable that it could be pushed completely across the middle line to the left side. At the consultation held upon the case it was suggested that the tumour might be a cystic enlargement of a floating kidney, a tumour at the head of the pancreas, or dropsy of the gall-bladder. No decided diagnosis was made, and Tait's proposal to open the abdomen and thus ascertain the nature of the tumour was agreed upon. On August 23, 1879, Tait opened the abdomen in the middle line to the extent of four inches, the umbilicus forming the centre of the incision. It soon became evident that the tumour was a gall-bladder, so Tait passed the needle of an aspirator into the apex and drew out ten ounces of fluid. He then opened the gall-bladder at the point of puncture so as to admit his

finger, and came at once upon a large round gall-stone lying loose in the cavity. This he removed and then found another at the entrance of the duct, impacted in it, and evidently the cause of the dropsical distension of the gall-bladder. This stone was difficult to remove, and Tait says:

> I thereupon performed a very careful and protracted lithotrity, chipping little fragments off the stone regularly all over its exposed surface till I had the satisfaction of lifting out its nucleus, I then passed the blade of a fine pair of forceps on each side of it, and by a gentle squeeze broke up the remainder and was then enabled to lift all out. . . . I washed the cavity out repeatedly, and took every precaution I could to be sure that no fragments were left. I then stitched the wound in the gall-bladder to the upper end of the abdominal wall by continuous sutures, leaving the aperture into the bladder quite open, and then I closed the rest of the abdominal opening in the usual way.

The patient made an excellent recovery.

Tait read a report of the case at a meeting of the Royal Medical and Chirurgical Society, November 11, 1879.

In the discussion that followed there was no enthusiasm. Tait was well aware of the importance of what he had done, and he was always careful to bring his best results to the Medico-Chirurgical Society, for he was always sure that the audience would contain a number of distinguished critics who would not spare him in the least if they saw a weak spot in his armour. He never expected praise, that would have been asking too much from men who simply hated the sight and name of Lawson Tait.

On this occasion one is struck by the fact that at least two of the speakers must have looked up the history of the operative treatment of the gall-bladder, and the first speaker, Hulke, brought to light, for the first time, the knowledge that Petit had published a memoir on the same subject, while he claimed the credit of first advocating such a procedure as cholecystotomy for Handfield-Jones.

Luckily for Tait none of the speakers appeared to know about Bobbs' case, or they could, and would, have used it

with much effect to take the wind out of the sails of this presumptuous provincial surgeon.

Henry Morris, in his remarks, said that in 1859 Thudichum suggested the same operation at a meeting of the Medical Society, when the President, Mr. Hilton, said that he thought the procedure justifiable, and referred to a case when such an operation had been done.

From what we have said above we know how much credit must be given to Petit, but the credit that has been bestowed on Handfield-Jones is not deserved. He had a case where there was obviously an obstruction of the common duct through a gall-stone becoming impacted in it, and he proposed to a surgeon that he should open the patient and try to work the stone down out of the duct into the duodenum. The surgeon refused, and after the patient died Jones tried at the post-mortem to put the plan into practice, but failed to move the stone. Upon this suggestion Handfield-Jones is hailed by Hulke as being the first to advocate such a procedure as cholecystotomy! The praise given to Morris for the suggestion in his paper at the Medical Society is also exaggerated. What he said in that paper was simply that, in some appropriate cases, an operation for the removal of gall-stones through the abdominal walls should become a subject of consideration for surgeons.[1]

Knowsley-Thornton, who was present at the meeting, and who was always "at daggers drawn" with Tait, asked the author of the paper to explain what he had meant in the concluding part of his communication when he had said that he always used rigid antiseptic precautions in his abdominal sections, but had expressed doubts as to his success being attributable in any to them. Tait on being questioned explained his views, and said that the success was not due to the antiseptic method *per se*. Thornton then observed that the fact still remained that the increasing success obtained by Tait followed his adoption of the antiseptic method.

Spencer Wells, who was also present, said that several

[1] The *Lancet*, October 22, 1859.

years before he had been consulted about the removal of gall-stones. At that time he considered that an opening by potassa fusa caustic would be preferable to the knife, and he should still, if such a case presented itself, prefer to adopt the former plan. This was certainly an extraordinary admission for a man to make who had done hundreds of ovariotomies, and it is in strange contrast to the following words by that great surgeon, Marion Sims, at the conclusion of his paper on his case of cholecystotomy :

> The great lesson which this case teaches us is this. In dropsy of the gall-bladder, in hydatid tumours of the liver, in suspected abscess of the liver, and in gall-stones, we should not wait till the patient's strength is exhausted, or until the blood becomes bile-poisoned, producing hæmorrhages, but we should make an early exploratory incision, ascertain the true nature of the disease, and then carry out the surgical treatment that the necessities of this case may demand.

We shall not here trace the further development of the surgery of the gall-bladder and ducts ; we shall do that in a future chapter. We shall merely record that Mayo Robson, a pupil of Tait, and a surgeon who did more to elucidate the complex problems of surgical affections of the gall-bladder and gall-ducts than any other surgeon in England, has said that " to Mr. Tait undoubtedly belongs the credit of having popularised the operation of cholecystotomy with the profession." [1]

Some evidence to support this statement is shown in the following table,[2] which gives all the cases before Tait's and those recorded for the first five years after Tait published his first operation :—

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[1] Mayo Robson, " On Gall-stones and their Treatment," 1892, p. 126.

[2] Buck, " Reference Handbook of Medical Sciences," vol. ii. p. 127.

TABLE OF CHOLECYSTOTOMIES, 1867–1884.

No.	Date of operation.	Age.	Sex.	Result.	Operator.	Remarks.
1	June 15, 1867	30	F.	Recovered	Bobbs	—
2	April 18, 1878	45	F.	Died	Sims	—
3	June 1, 1878	30	F.	Recovered	Kocher	—
4	November, 4, 1878	60	F.	Died	Keen	—
5	August 23, 1879	40	F.	Recovered	Tait	—
6	October 9, 1881	55	F.	Recovered	Tait	—
7	January 15, 1882	24	F.	Recovered	Tait	—
8	1882	34	M.	Recovered	Tait	—
9	1882	47	F.	Recovered	König	—
10	October 13, 1882	39	F.	Recovered	Tait	—
11	October 31, 1882	53	F.	Recovered	Eddowes	—
12	November 24, 1882	47	F.	Died	Trendelenberg	—
13	January 5, 1883	28	F.	Recovered	Tait	—
14	January 30, 1883	59	F.	Died	Meredith	Patient recovered from operation
15	April 20, 1883	59	F.	Died	Gross	—
16	May 3, 1883	76	M.	Died	Rausohoff	—
17	May 6, 1883	35	F.	Recovered	Tait	—
18	May 10, 1883	42	F.	Recovered	Tait	—
19	May 28, 1883	66	M.	Died	Tait	Patient recovered from operation
20	June 19, 1883	31	M.	Died	Musser & Keen	—
21	October 31, 1883	38	F.	Recovered	Gardiner	—
22	October 6, 1883	Old	M.	Recovered	Savage	—
23	November 14, 1883	44	F.	Recovered	Tait	—
24	December 20, 1883	44	F.	Recovered	Tait	—
25	May 8, 1884	62	F.	Recovered	Tait	—
26	June 26, 1884	45	F.	Recovered	Tait	—
27	August 6, 1884	73	M.	Recovered	Tait	—

We think that a perusal of the facts brought out by the history of the development of the operation of cholecystotomy shows that the real founders of gall-bladder surgery were Marion Sims and Lawson Tait. But it is very evident that the Americans look upon Bobbs as the founder, for at the 1905 Portland Session of the American Medical Association, it was proposed to exhibit Mrs. J. Burnsworth, McCordsville, Indiana, who was operated on June 15, 1867, by Dr. John S. Bobbs. It was also proposed to distribute a reprint from the *Indiana Medical Journal* containing a history of the first operation, and sketches of Dr. Bobbs and the patient; the reprint to bear the following dedication :

This reprint is dedicated to the American Medical Association, in session at Portland, Oregon, June 11–14, 1905, in memory of Dr. John S. Bobbs, of Indianapolis, the founder of the surgery of the gall-bladder, by the Medical College of Indiana, the *Indiana Medical Journal*, and the Indianapolis and State Medical Societies, which Dr. Bobbs assisted in organising, and on behalf of the thousands of patients whose lives have been made happy and prolonged by this beneficent operation.[1]

Since we wrote the above notes we have come across the following words of Ochsner: "Lawson Tait in 1879 performed the first successful operation of cholecystotomy, and to Tait, more than to any other surgeon of this period, is due the credit of placing the surgery of the liver and gall-bladder on a firm basis.[2]

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[1] *Jour. A. M. A.*, July 8, 1905, p. 118.
[2] Kelly and Noble, "Gynæcology and Abdominal Surgery," 1908, vol. ii. p. 277.

XII

Tait's Operations for Repair of the Female Perineum

1879

At the same meeting of the Obstetrical Society, held on November 5, 1879, at which Tait read notes on his apparatus for dilating the uterine canal by continuous elastic pressure, he also read his paper "On New Methods of Operation for Repair of the Female Perineum."

Tait began his communication by saying that:

> "the admirable papers of Dr. Mathews Duncan leave nothing to be said upon the condition of the perineum, when it is in need of repair, or upon the causes which produce them. A very large experience of operation of the perineum has, however, led me to desert the methods of operation hitherto adopted, and which are recommended by Dr. Duncan, and I now wish to describe methods which have proved to me to be superior to the old, and which are so essentially different from anything I have seen described, as fairly to merit the name of *new*.

Tait then proceeds to say that the cases that come for repair are divided by him into two classes, those (1) in which the laceration is complete, and (2) in which it is incomplete. In the first class he ranked all those in which the damages resulted in an inability to retain the fæces; in the second, he included those in which fæces can be retained, even if the power of retention be slight.

Later on he says:

> The principles upon which my new method of operating is based are chiefly two. The first is that no tissue is removed, flaps being lifted only, so that if the operation fails they go back to their original position, and the patient is left just as she was, without detriment to the success of a repeated effort, as is the case if edges are pared. The second principle applies to the method of application of the stitches. Hitherto these have been recommended to be applied through the thickness

> of the flap, and in an interrupted fashion like skin sutures. In my method they are applied in the axis of the wound, and so as to be practically continuous.

Tait then gives a description of his two operations, and his paper is illustrated by two diagrams. The descriptions of the operations, we have no hesitation in saying, are so bad that no one could possibly perform either operation properly after reading the paper and examining the diagrams.

At the meeting, Mathews Duncan said that he had not been able to follow all the details; Rogers said that he regretted not having grasped, or understood, all the suggestions of the paper, and did not clearly follow the demonstration of the author; Tait, when he came to reply, said that he " confessed he was at a disadvantage in endeavouring to explain the steps of the operation, because all plastic operations were extremely difficult to describe. He seemed either to have lacked precision in describing his proceeding, or some of the Fellows could not have been closely following the reading of the paper by the secretary. He had no doubt that when the paper was in print, he would be able to follow the steps of the operation, aided by the illustrations which would be given."

Neither the description given in Tait's paper at this time, nor in the fuller description given in his book (" Diseases of Women and Abdominal Surgery," 1889, p. 68), made his operations clear; and Tait has paid us the compliment of saying at a later date, when he wrote the preface for a small work, which we published about his Perineal Operations: [1]

> I complied with the request of my friend, Dr. Stewart McKay, to read the following pages and a few notes with much pleasure, because his description of my perineum operations are the first which I have seen full enough to be of any use to those desirous of doing these operations, as I wish them to be done. This is probably to be attributed to a want of fullness or clearness in my own description.

Before giving a description of these operations, it will be well to point out that the first of these procedures, which

[1] Stewart McKay, " Lawson Tait's Perineal Operations, and an Essay on Curettage of the Uterus," London, 1897.

we call the " V " operation for incomplete rupture of the perineum, is in our opinion an operation of so little use, that no one need bother about it ; and we shall show later on that it is nothing more than a copy of an operation devised years before by Baker Brown.

The second operation, which we term the " H " operation, is, as far as we have been able to ascertain, quite an original piece of work, and in our opinion it is one of the most ingenious, and the most original, operations that Tait has left us. After practising this operation for twenty-five years, we never perform it without admiring the wonderful ingenuity and originality of the whole procedure. We have, during the last ten years, added a few steps to Tait's original plan ; but we doubt if our great master would have approved of these additions to his work. We shall, therefore, give a description, with his own notes, of the way that he did the operation when we were with him in Birmingham, and then we will describe the steps that we have added in performing the " H " operation.

Lawson Tait's Perineorraphy

Preparation of the Patient

The bowels should be well opened the day before the operation, and on the morning of the operation an enema should be given.

The skin about the vulva must be shaved and well washed with soap and water.

The patient is placed in the lithotomy position on a Kelly rubber pad.

Instruments Required

A pair of angular scissors ; the lower blade must have a sharp extremity.

A pair of dissecting forceps, and a dozen pairs of pressure forceps.

A curved needle and needle-holder.[1]

Strong silkworm gut for sutures.

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[1] Tait always used a handled needle.

Clover's crutch, and irrigator, cotton-wool, and swabs.
Fine catgut for ligatures.
Dressing and a " T " bandage.

Description of the V Operation for Incomplete Rupture.[1]

This operation is known as the " V " operation, in contradistinction to the other, which is called the " H "; these letters representing roughly the shape of the incisions made in performing the operation.

First Step : The Incision (Fig. 1).—The sharp point of the lower blade of the angular scissors is pushed into the tissues at MP, which is the anterior extremity of what remains of the median raphé of the perineum, P. The blade sinks for ¾ inch into the tissue, and it is well to insert the index finger of the left hand into the rectum, so as to steady the parts and prevent the blade entering the bowel. A cut is now made upwards along the junction of the skin, S, and the vaginal mucous membrane, PVF, to the point R, situated internal to the distal extremity of the right labium minus. A similar cut is made on the left side to L. A preliminary and vertical snip in the original direction of the raphé enables the operator to make a regular and symmetrical start for the two sides.

Second step : The Reflection of the Vaginal Wall.—The incision having been made, we find that the mucous membrane of the posterior vaginal wall, PVF, being now liberated, retracts, and we thus come to have formed a raw V-shaped bleeding surface, RS (Fig. 2). In order that we may increase this surface, it is necessary to gently snip round the edges of PVF, and by so doing we really make a kind of flap of mucous membrane, PVF (Fig. 2). The more we snip, the more this flap retracts, being liberated from the underlying tissues, and the larger becomes our V-shaped raw surface. Mr. Tait most emphatically says that this surface, RS, should not be a large one, and he seldom did more than make the primary incision. If we dissect the flap up too much,

[1] This description was corrected by Mr. Lawson Tait, and he has added a note here and there where necessary.

FIG. 1.—THE V OPERATION FOR INCOMPLETE RUPTURE: FIRST STEP—THE INCISION. (FROM SKETCH BY THE AUTHOR.)

This figure represents an incompletely-ruptured perineum—*i.e.*, the rupture has not extended into the rectum. *P* is the remains of the perineum; *MP*, the anterior extremity of the median raphé; *R* and *L*, right and left sides, the letters being placed internal to the distal ends of the labia minora, *LM*; *CL*, the clitoris. The dark lines running from *MP* to *R* and *L* are seen to be lying at the junction of *S*, the skin, and the mucous membrane of the posterior vaginal wall, *PVF*, and these dark lines represent the line of incision. *AV* is the anterior vaginal wall; *U*, the meatus urinarius; *AN*, the anus.

To face page 122.

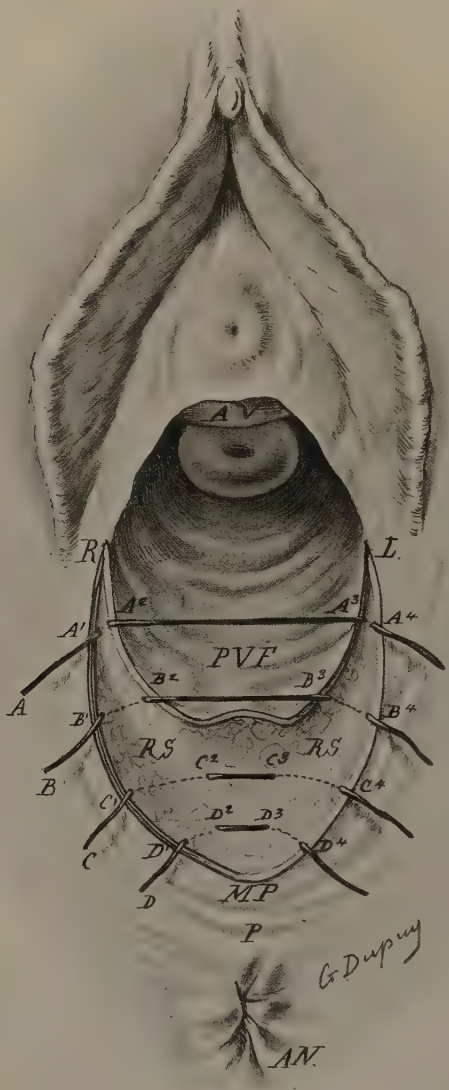

FIG. 2.—THE V OPERATION FOR INCOMPLETE RUPTURE: SECOND AND THIRD STEPS. (FROM SKETCH BY THE AUTHOR.)

The V-shaped incision that was marked out in Fig. 1 has now been made, and as the result the posterior vaginal wall, *PVF*, has retracted, and a raw surface, *RS*, remains. The raw surface is increased by snipping under the flap of mucous membrane, *PVF*, allowing it to retract still more. The four sutures, *A*, *B*, *C*, *D*, which are usually inserted are shown in position.

To follow Plate X.

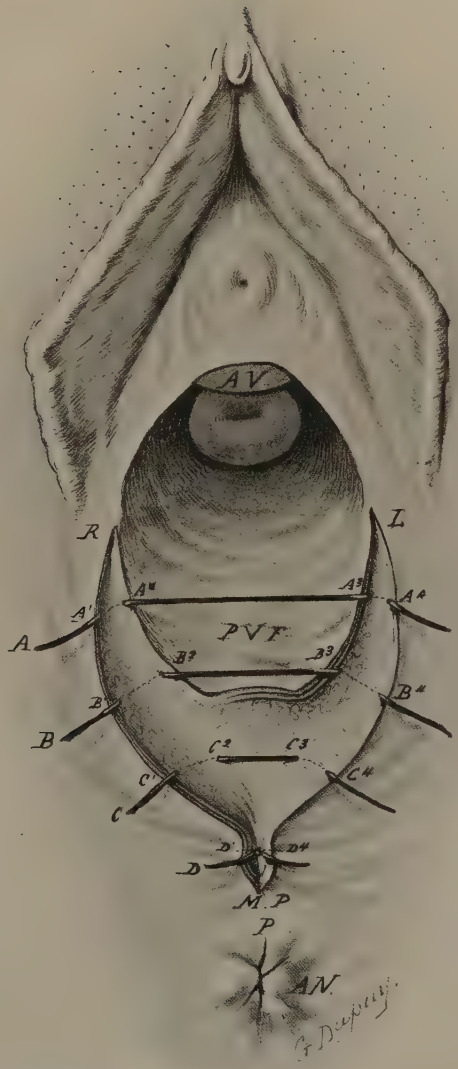

FIG. 3.—THE V OPERATION FOR INCOMPLETE RUPTURE: FOURTH STEP—TYING THE SUTURES. (FROM SKETCH BY THE AUTHOR.)

The lowest stitch, *D*, shown in Fig. 2 has now been tied. The figure shows that the skin edges are turned out because the suture has been inserted *inside* the skin edge, not *through* the skin itself.

To follow Plate XI.

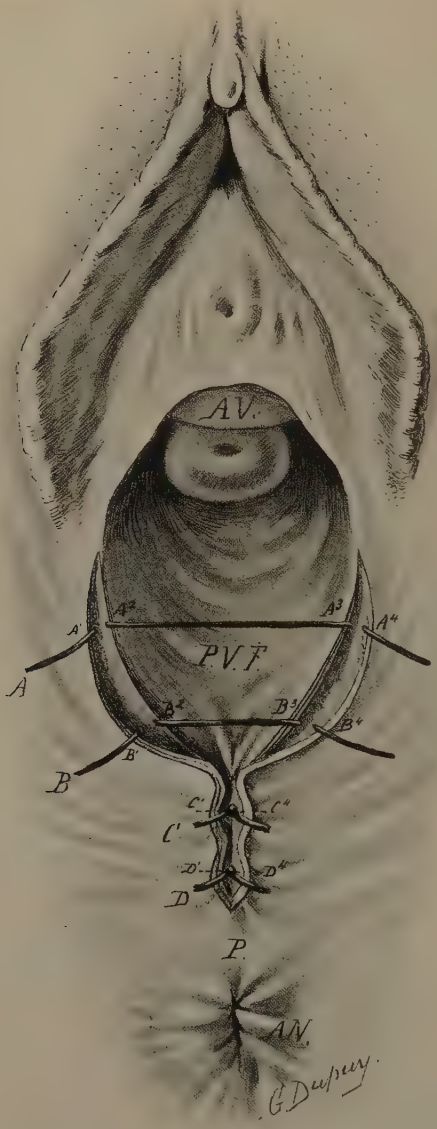

FIG. 4.—THE V OPERATION FOR INCOMPLETE RUPTURE: TYING THE SUTURES. (FROM SKETCH BY THE AUTHOR.)

D and *C* have both been tied; *B* and *A* remain to be tied. In order to accomplish this, it is necessary to bring the surface $A^1\ A^2\ B^2\ B^1$ into apposition with the surface $A^4\ A^3\ B^3\ B^4$ in the median line, *in front of* the posterior vaginal wall, *PVF*.

To follow Plate XII.

we are only weakening the parts and defeating the primary idea of the operation, which is not a "denuding" one, but a "flap-splitting" one. Too extensive an incision completely bisects the perineal mass, which is not advisable.

Third Step: Insertion of the Sutures (Fig. 2).—Taking the needle, we bury the point at the spot marked D4, which is on the left of the patient, and is *just within* the skin edge. Keeping the left index finger in the rectum, we push the point of the needle well into the tissues below the raw surface, RS, and then cause it to emerge at D3, near to the median line. When it has emerged, we carry the point over the median line, and bury it at D2, and pushing it deeply below the raw surface, we cause it to emerge at D1, just inside the skin edge.

Suture C is inserted in the same manner as suture D.

Mr. Tait always inserted the needle *inside* the skin edge, not through the skin, so that when the sutures are tied the skin edges will be turned out, and a good median raphé will be the result.

Sutures B and A are inserted differently to C and D. Insert the needle at B4, and bring out the point at B3, just *external* to the edge of the posterior vaginal flap, PVF; now carry the needle across *in front of* PVF, and bury the point at B2 and bring it out at B1.

It is well, after having inserted each stitch, to seize each end with a pair of catch forceps; this prevents the sutures being accidentally pulled out, and by allowing the forceps to hang down over the anal region they keep the sutures out of the way.

Fourth Step: Tying the Sutures (Figs. 3, 4, 5).—First seize the six catch forceps attached to the extremities of sutures A, B, C, and hand them to an assistant, who holds them up over the mons. Now irrigate the raw surface with a hot solution to cleanse the parts and to stop any oozing. If any vessels require tying, these should be attended to.[1]

Seizing the two catch forceps attached to the extremities

[1] I use no cleansing but a sponge and plain water, and always have found the proper fastening of the sutures sufficient to arrest the surface bleeding.—LAWSON TAIT.

of suture D, we pull gently on each, the assistant on either side pressing each buttock in towards the median line; by this means the lower sides of the V are brought into apposition. The suture is then tied (Fig. 3).

Suture C is tied in the same way (Fig. 4).

Sutures A and B now remain. The assistants press still more strongly on the sides R and L, so as to bring the surface A1, A2, B2, B1 (Fig. 4) into apposition with A4, A3, B3, B4 in the median line *in front of* PVF.

Fig. 5 shows the appearance of the parts when all the sutures have been tied.

Readers of this description of the operation frequently ask, What becomes of the flap PVF? The answer is: If the incision is made properly, no real flap will be formed; but if too much dissection has been practised in order to get a large, raw surface, then a flap is formed, and this adheres to the anterior face of the newly restored perineum.

Description of the H Operation for Complete Rupture of the Perineum

For the proper understanding of this operation,[1] I lay great stress on the peculiar mechanism of the original rent and its method of healing. It is the only known wound whose cicatrix is inevitably and constantly at right angles to the direction of the wound from which it results. This is due to the continuous dragging of the divided sphincter. It should be noticed that the tear of the perineum is always parallel with, if not actually in the line of, the raphé, whilst the cicatrix is parallel with the septum, the two halves of the perineum being dragged apart and buried in the divided skin and mucous membrane as deeply as possible, carrying with them the separated ends of the sphincter ring. For proper repair, their separated ends must be exposed, and therefore the laterals of the incision must be deep.

Before making the incision, the cicatrix must be carefully and completely stretched by the thumbs of the operator, being firmly fixed outside the anal margins, and separated until the clear white line of the cicatrix starts into view (Fig. 6). This will be found to consist of a horizontal line along

[1] This was added by Lawson Tait after reading our description.

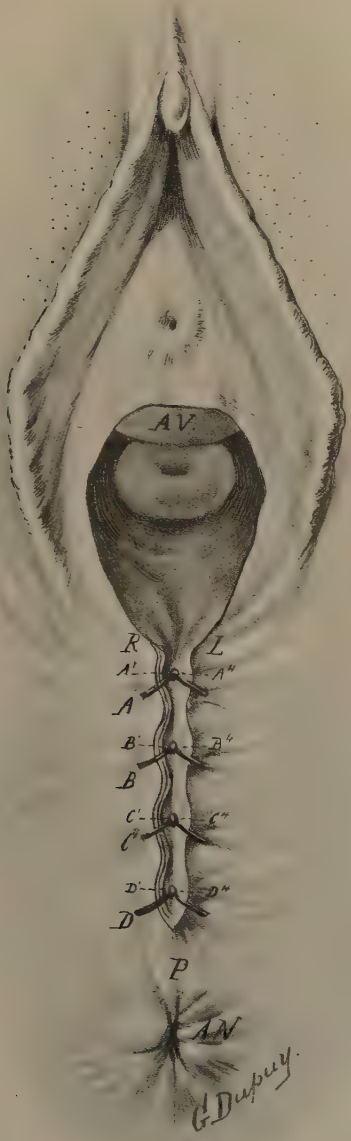

FIG. 5.—THE V OPERATION FOR INCOMPLETE RUPTURE: TYING THE SUTURES. (FROM SKETCH BY THE AUTHOR.)

This figure shows the position of the parts when all the sutures have been tied.

To face page 124.

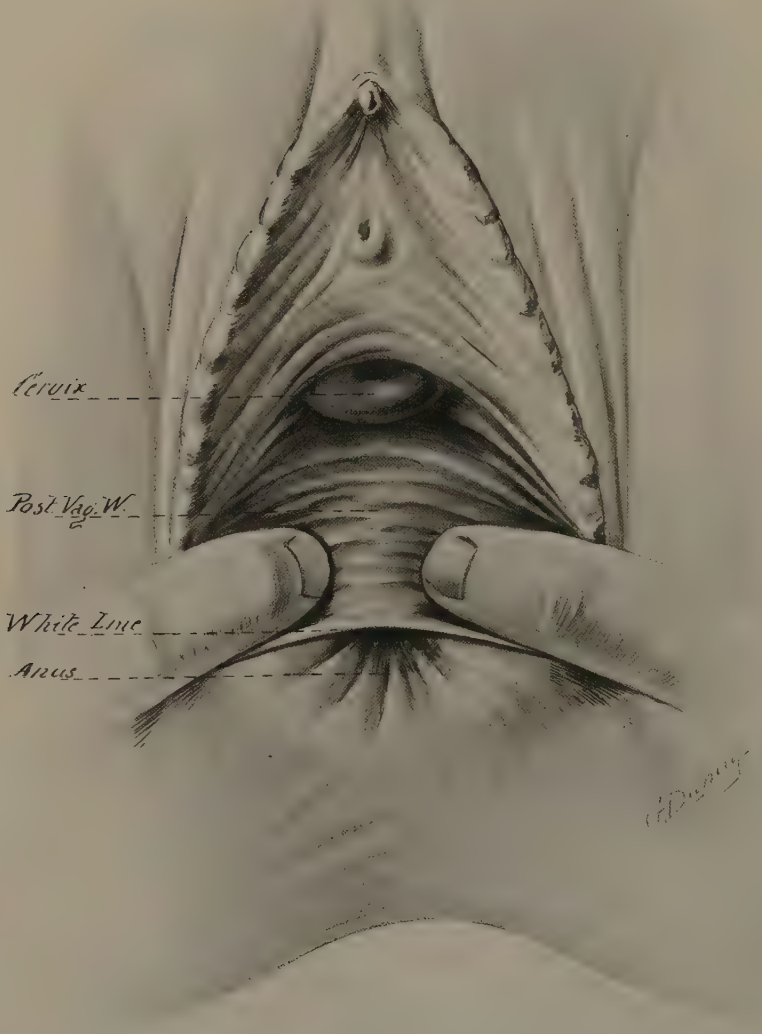

FIG. 6.—SHOWING THE LINE OF THE CICATRIX BETWEEN THE VAGINA AND RECTUM. (AFTER REED.)

To follow Plate XIV.

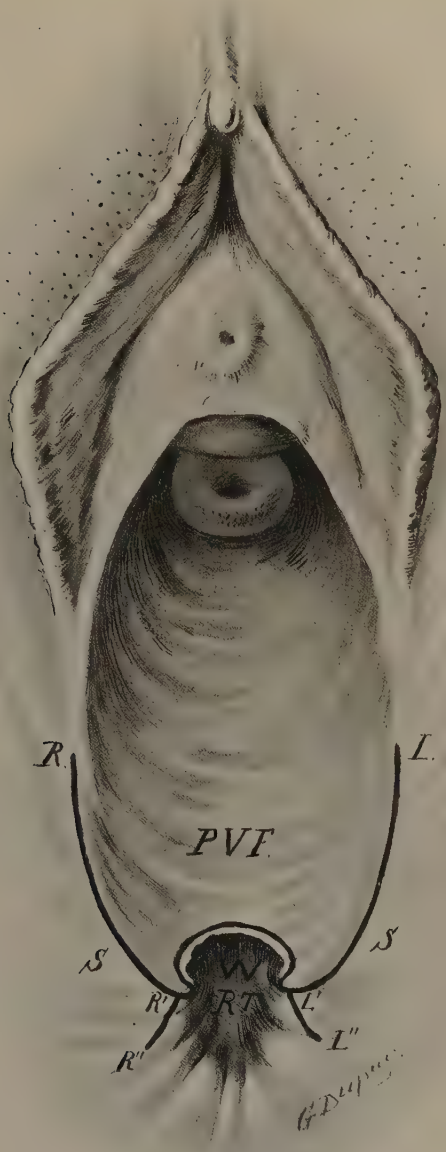

FIG. 7.—THE H OPERATION FOR COMPLETE RUPTURE OF THE PERINEUM : THE INCISIONS. (FROM SKETCH BY THE AUTHOR.)

This figure represents the state of things when the perineum has been completely ruptured into the rectum, and the rupture has extended some little distance up the rectum. The junction of the posterior vaginal mucous membrane, *PVF*, with the rectal mucous membrane is represented by a white line. The various lines of incision are shown in this figure and in Fig. 8. The point of the scissors is inserted at *L'*, and an incision is made round the concave white line to the point *R'*, and from *R'* to *R*, then from *L'* to *L*, then from *L'* to *L''*, and from *R'* to *R''*.

To follow Plate XV.

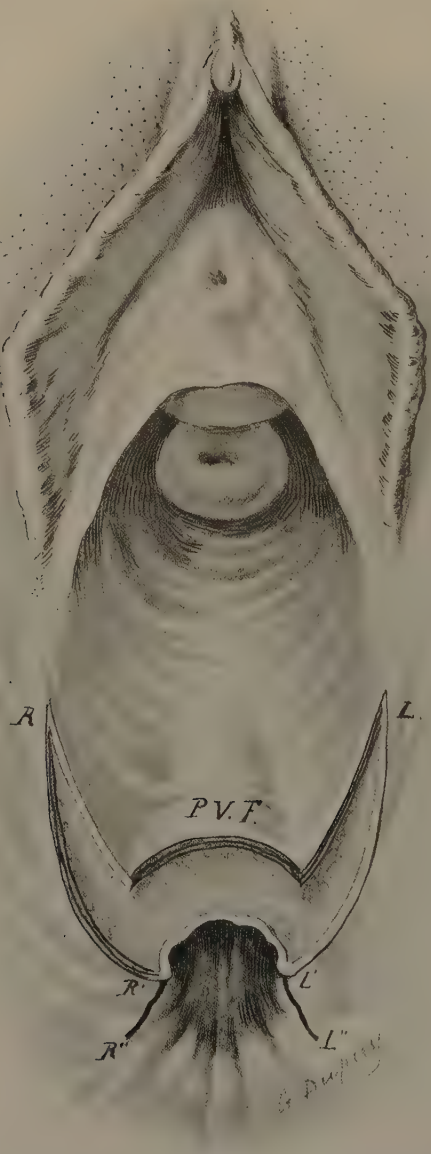

FIG. 8.—THE H OPERATION FOR COMPLETE RUPTURE OF THE PERINEUM: THE REFLECTION OF THE VAGINAL WALL. (FROM SKETCH BY THE AUTHOR.)

Showing how the posterior vaginal mucous membrane, *PVF*, retracts when the incisions are made from *L'* to *R'*, from *R'* to *R*, and from *L'* to *L*. The incisions from *R'* to *R''*, and from *L'* to *L''*, have not yet been made.

To follow Plate XVI.

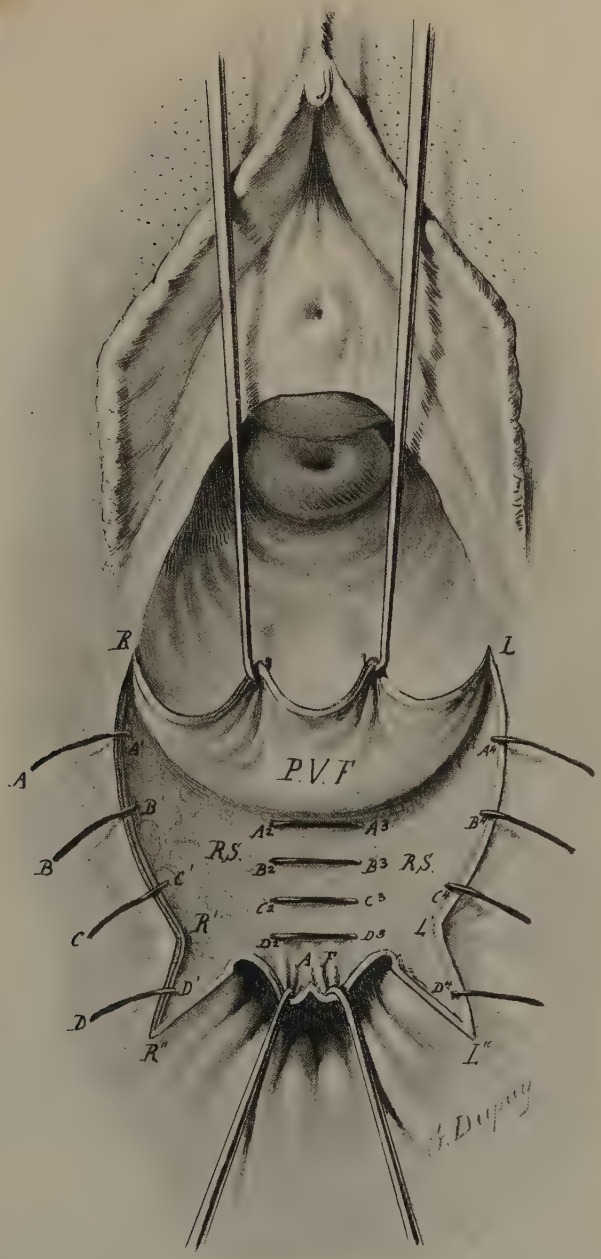

FIG. 9.—SHOWING THE POSTERIOR VAGINAL FLAP DISSECTED UP AND HELD BY TWO HOOKS. (FROM SKETCH BY THE AUTHOR.)

The incisions from L' to L'', and from R' to R'', have now been completed, and the parts have gaped outwards. The figure also shows how the rectal flap is formed by splitting the free edge of the tissue that borders on the rectal mucous membrane.

To face page 126.

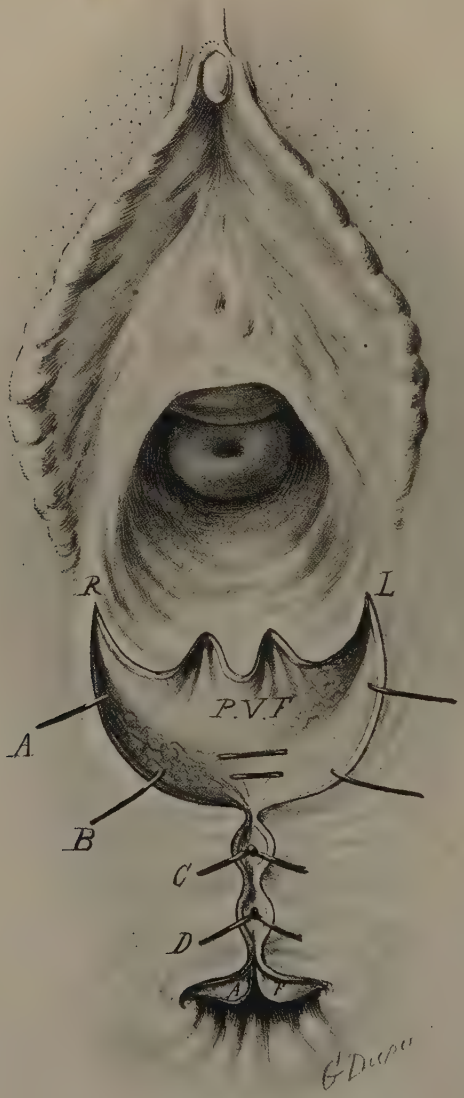

FIG. 10.—FOURTH STEP: TYING THE SUTURES. (FROM SKETCH BY THE AUTHOR.)

The two lower sutures, *D* and *C*, have now been tied, and the anal flap, *AF*, is shown in position. Sutures *B* and *A* still remain to be tied.

To follow Plate XVIII.

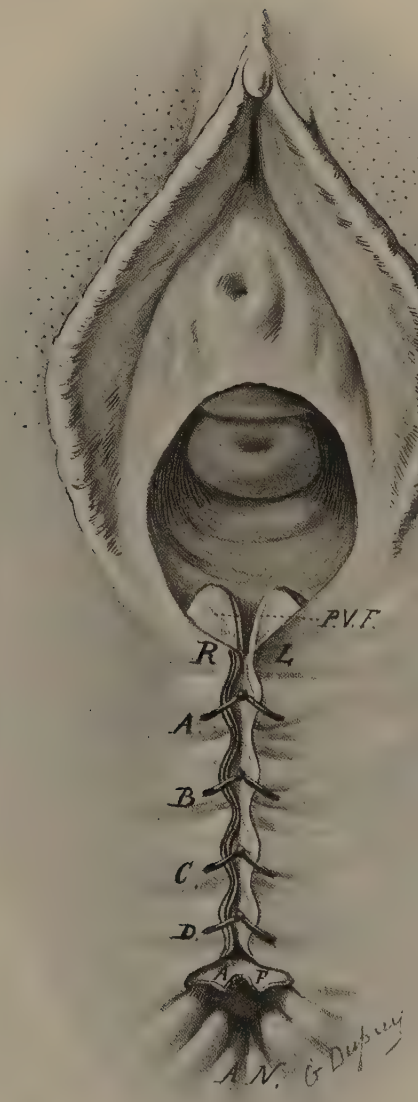

FIG. 11.—STATE OF THE PARTS WHEN THE SUTURES HAVE BEEN TIED.
(FROM SKETCH BY THE AUTHOR.)

All the sutures have been tied, and the flap of posterior vaginal mucous membrane, *PVF*, is left to fall against the posterior surface of the newly-formed perineum.

To follow Plate XIX.

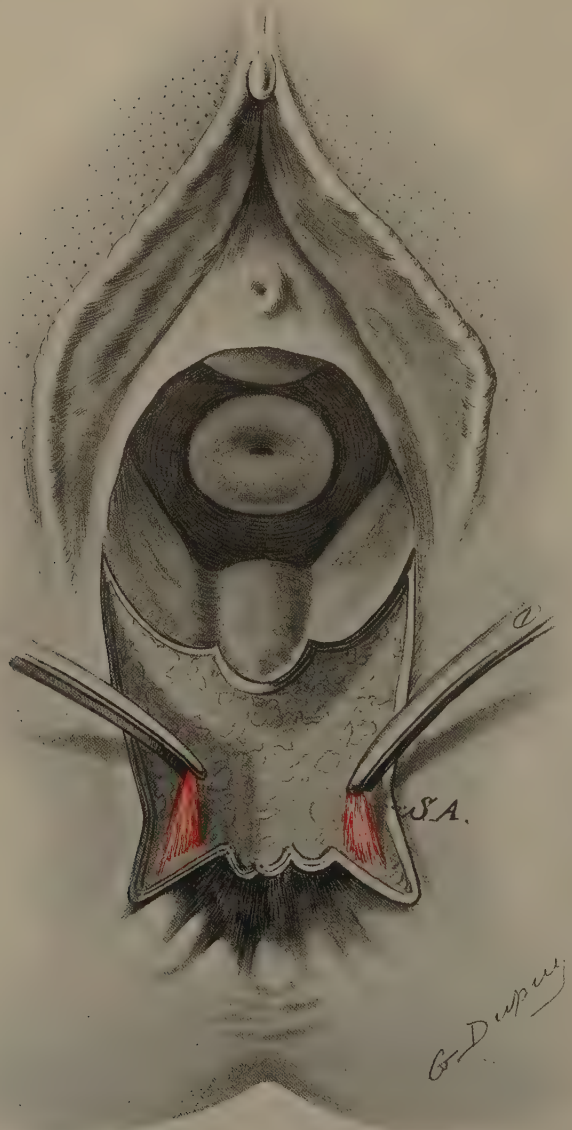

FIG. 12.—COMPLETE RUPTURE OF PERINEUM: REPAIR BY TAIT'S OPERATION. (FROM SKETCH BY THE AUTHOR.)

The torn ends of the sphincter ani, *SA*, are dissected out.

To follow Plate XX.

PLATE XXII.

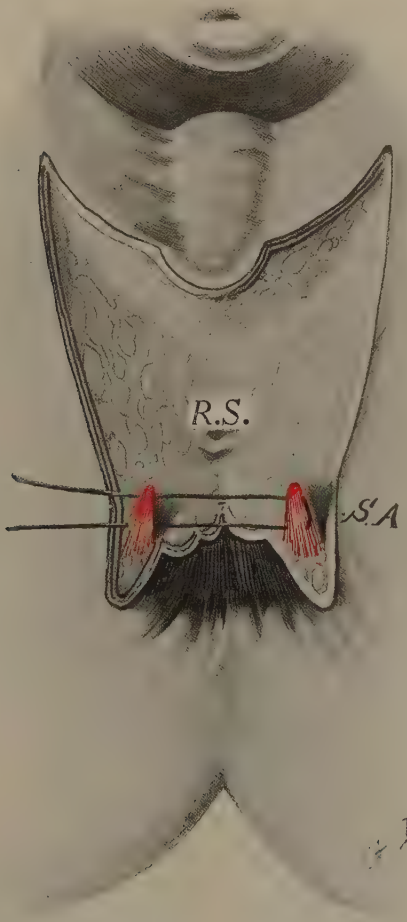

FIG. 13.—COMPLETE RUPTURE OF PERINEUM: REPAIR BY TAIT'S OPERATION. (FROM SKETCH BY THE AUTHOR.)

The torn ends of the sphincter ani, *SA*, are dissected out and a mattress suture passed to unite them.

To follow Plate XXI.

the edge of the rupture and two shorter vertical lines, one at each end of the horizontal line, making a very wide H, like this :

)——(

The points where the transverse line cuts the vertical lines indicate the burial-place of the separated ends of the sphincter, and here the scissors must enter deeply at one end, and travel carefully along the horizontal white line till they reach the other point of juncture ; then an upward and downward snip completes the incision. The rest, in Dr. McKay's words, makes the process quite plain.—LAWSON TAIT.

First Step : The Incisions (Fig. 7).—The sharp point of the lower blade of the angular scissors is pushed into the tissues at L′ ; the blade is made to sink for ½ inch into the tissues, and the left index finger is placed in the rectum beneath the " white line "—*i.e.* beneath the junction of the vaginal with the rectal mucous membrane. A cut is now made along the white line from L′ to R′, then from R′ to R—*i.e.* along the junction of the skin, S, and the vaginal mucous membrane, PVF. The next incision is made from L′ to L, then from L′ to L″, and then from R′ to R″.

Second Step : The Reflection of the Vaginal Wall (Fig. 8).—The incisions from L′ to R′, from R′ to R, and from L′ to L, having been made, we find that the mucous membrane of the posterior vaginal wall, PVF, being now liberated, retracts, and we thus come to have formed a raw W-shaped surface (Fig. 8). In order that we may increase the surface, it is necessary to snip gently round the edge of PVF, and by so doing we make a real flap of mucous membrane, PVF, from the posterior vaginal wall. It is necessary to snip under this until we have a fairly large raw surface (Fig. 9).

In order that we may gain a still larger raw surface, we now complete our incision from R′ to R″ and from L′ to L″.

Fig. 9 shows the appearance of the parts when these incisions are completed.[1]

Third Step : Insertion of the Sutures (Fig. 9).—Before inserting the sutures, it is necessary to insert two hooks into

[1] Mr. Tait used not to dissect out the ends of the sphincter ani muscle ; this we consider should be the next step of the operation, and have added a note at the end of this description showing how this is done.

the posterior vaginal flap, PVF, and pull it up out of the field of operation. Two hooks are likewise inserted into the free border of the rectal mucous membrane, and by pulling these downwards we have formed the anal flap (Fig. 9, AF). Taking a needle we bury the point at the spot marked D4 (Fig. 9), which is on the left of the patient, and is just inside the skin edge. Keeping the left index finger in the rectum, we push the point of the needle well into the tissues below the raw surface, RS, and then cause it to emerge at D3, near to the median line. The point is carried over the median line and buried at D2, then under the raw surface, to emerge at D1. The other sutures are inserted in the same way.

Fourth Step : Tying the Sutures (Figs. 10, 11).—First seize the six catch forceps attached to the extremities of sutures A, B, C and hand them to an assistant, who holds them up over the mons. Now irrigate the raw surface with a hot solution to cleanse the parts and to stop any oozing. If any vessels require tying, these should be attended to. Seizing the two catch forceps attached to the extremity of suture D, we pull gently on each, the assistant on either side pressing each buttock well in towards the median line, while one of the assistants makes gentle traction on the anal flap, AF (Fig. 9).

The lowest suture is tied first. Great care must be taken that the sutures are not tied too tightly. It is a good plan, if we employ silk-worm gut, to tie them all with the first loop of the "surgeon's knot," and then to go over them again and complete the second loop of the knot ; this enables us to loosen any of the sutures that have been pulled too tight in the first instance.

Fig. 11 shows the state of the parts when the sutures are tied. It also shows how the flap PVF, formed from the mucous membrane of the posterior vaginal wall, will fall against the newly-made perineum, where it will adhere and help to strengthen the perineum.

After-Treatment

After completing the operation, some dressings and a T-bandage are applied. [I do not apply any dressings, but

direct the nurse to keep the parts quite dry.—LAWSON TAIT.]

The patient's legs should be tied together until she has regained consciousness, else she may struggle and put great tension on the sutures.

The bowels should be confined for two days, then a dose of castor oil may be given, and the bowels washed out by an enema. It is a mistake to keep the bowels confined for four days, as the fæces become so hard that the patient may tear the stitches away while straining at stool.

The external genitals should be douched with hot boracic solution three times a day.

No matter how unfavourable the parts may look during the first fortnight, it is wise not to remove the sutures until the third week.

Suturing the Ends of the Sphincter Ani.

We have left the description of this operation as it appeared in the first edition of our work, because it was the exact way in which Mr. Tait desired the operation to be performed. For some years, however, we have added a step to the operation, inasmuch as we now dissect out the ends of the sphincter ani, and suture these ends together with catgut, before we insert the chromic gut sutures. By this means we make more certain that the muscle ends will come into direct apposition.

After the completion of the second step, as described above, we proceed to dissect out the ends of the sphincter.

In order that one may not divide or mutilate these ends unnecessarily, it is wise to make the incisions from R′ to R″ and from L to L″ (Fig. 8) not too deep. The end of the muscle on either side is difficult to differentiate from the surrounding tissues. The best way to find the ends is to follow the convex swelling that the muscle makes round the anus. This swelling is from ¼ to ½ inch in width, and by following this ridge up to the incision the operator gains a much better idea of the place where he is to seek for the muscle end.

Each end having been dissected out, it is then held up with a tenaculum forceps (Fig. 12). The ends should be

freed for at least ½ inch, so that they will meet in the median line without tension.

When the dissection is complete, a catgut suture (No. 2 plain) is inserted on the mattress plan (Fig. 13). A second or a third suture is always inserted so that the muscle ends are not only united but are sewn into the raw perineal surface about the median line (Fig. 13, RS).

When the muscle ends have been brought together, it will be found not infrequently that the anal outlet is so small that defecation will only be performed with difficulty and at great risk of parting the newly joined ends of the muscle. We have, therefore, adopted the very simple, but very effective, plan of inserting a tenotomy knife and dividing the sphincter at a spot in the median line directly opposite to where the muscle ends are joined anteriorly. The effect of this is the immediate relief of the tension at the new junction, and in a short time the muscle fibres are cemented together posteriorly where the subcutaneous incision has been made in the median line.

Lastly, we found it of great advantage when the operation is completed, to insert into the rectum, from two to three inches, a rubber tube the size of the index finger. Through this twice a day the bowel is washed out ; the flatus passes readily and the patient is at ease.

The tube is removed on the fourth day, after which a dose of castor oil is given.

We have in recent years ceased to employ silk-worm gut in this operation, as we found that chromic gut (No. 2) does not cut through the tissues and gives much less pain. We pass these sutures through the skin—not within the skin edge as Tait used to do.

Only once in the last twenty years have we failed with this operation, and that was in a case when another operator had made several attempts to restore the sphincter action, but having failed he had destroyed so much of the muscle that we failed to get a good result when we attempted to carry out this operation.

We do not propose to give the history of the evolution of operations on the perineum, but we wish to draw attention

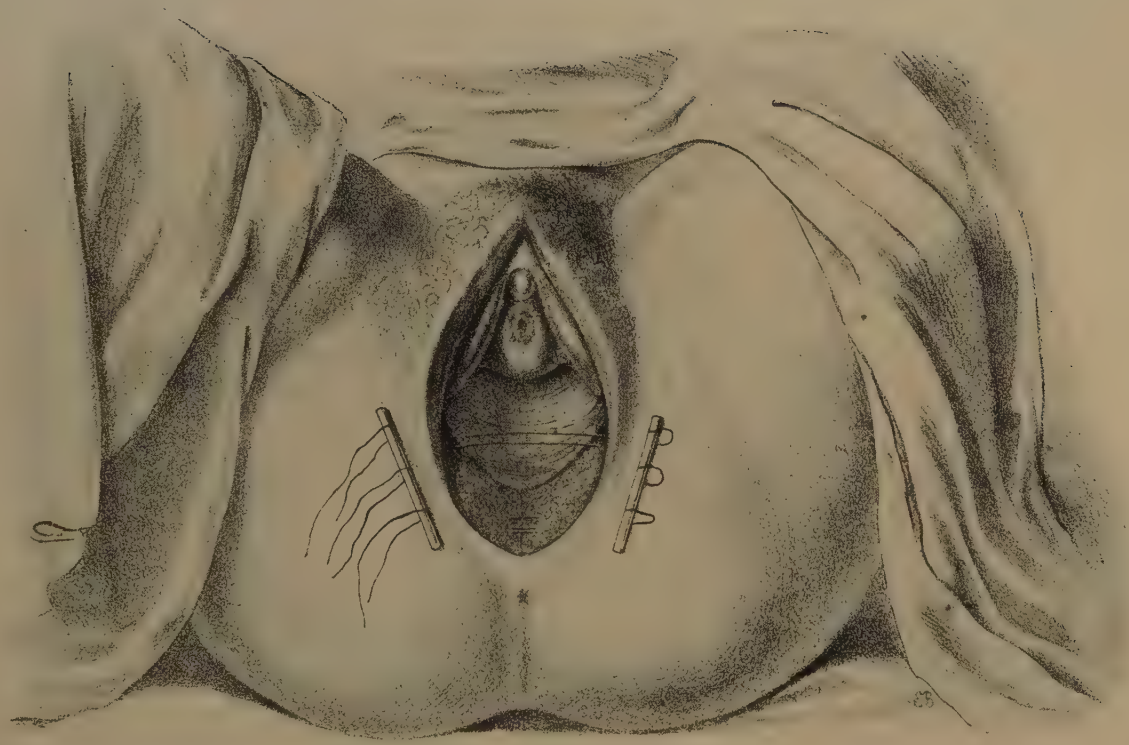

FIG. 14.—OPERATION FOR PROLAPSUS UTERI, SHOWING THE DENUDED SURFACES AND THE INSERTION OF THE QUILL SUTURES, BEFORE THE EDGES ARE BROUGHT TOGETHER. (FROM BAKER BROWN'S "SURGICAL DISEASES OF WOMEN," 1861.)

To face page 129.

to a figure (Fig. 14) published by Baker Brown in his " Surgical Diseases of Women," in 1861, where the raw surface is similar in outline to Tait's " V " operation. There was this difference between Baker Brown's operation and Tait's, that the latter obtained his raw surface by flap-splitting, while Brown got it by denudation ; the latter also used ivory bars, one on each side of the wound—called quill sutures—for the deep sutures as shown in the figures. Notice also that the upper sutures pass in front of the vaginal wall as in Tait's operation.

XIII

Tait's Uterine Dilator

1879

How the cervical canal can be dilated sufficiently to allow the interior of the uterus to be explored, or to be operated on, or to be treated by therapeutic applications, is a problem that has engaged the attention of gynæcologists since the days of Hippocrates.

Let us compare the methods that have been proposed and practised before Tait's day, with the method that he proposed, and see if his idea had any advantages.

Among the works that have been handed down to us bearing the name of Hippocrates, there are some which are undoubtedly the "genuine works of Hippocrates," while there are many which are definitely proved by ancient and modern authors not to have been written by him; among the latter we find a particular series, supposed to have been collected after the time of Hippocrates, but previous to the time of Aristotle, and these works deal specially with gynæcology. In some of these books we find the first descriptions of the methods adopted by the ancients for dilating the cervix.

The method of dilation described by the author of the Hippocratic treatise is as follows: [1] The patient first received fumigations for five or six days until the cervix was thought to be in a suitable condition. After the fumigation, dilators (*τῶν προσθετῶν*) made of pieces of very greasy pine-wood were introduced into the cervix. There were six dilators, and each in length was six finger-breadths; they ended in a point, and each succeeding rod was a little larger than the preceding one, the largest being the size of the index finger, and shaped like a finger, being smaller at the distal

[1] Stewart McKay, "History of Ancient Gynæcology," 1901, p. 263.

extremity. Before being introduced they were smeared with oil. At first the point was introduced gradually by turning the dilator round and pushing it at the same time, the rod being introduced for a distance of four finger-breadths (2·8 inch). After the first rod had been introduced it had to be withdrawn, and replaced by a larger one. During the after-treatment a leaden pipe filled with mutton fat, was kept in the uterus during the night time, while in the day-time one of the pine rods was used.

Aetius, quoting Archigenes, gives us an excellent account of the use of sponge tents in dilating the cervix. He says :

> When we find that the parts are soft to the touch we should introduce pieces of dry sponge, to which are attached pieces of thread, in order that they may dilate the narrow portion of the canal; and if they should slip out, then we should place larger ones in, and for this reason it behoves us to have several different sizes ready.

In the Hippocratic treatise mentioned above, gradual dilation of the cervix by means of tents made of raw flax, is mentioned. There are three of these tents, the first small, the second a little larger, and the third as large as the little finger, and in length four finger-breadths.

With regard to uterine dilation by means of instruments, whose blades are separated after introduction, and which are first recommended by Busch for obstetrical purposes, we find no mention of such among the ancient authors until we come to the work of Albucasius, of the twelfth century, and then we find these instruments described and used for dilating the cervix in obstetrical cases.[1]

Sir James Simpson is generally credited with the first introduction, in 1844, of sponge tents for gynæcological purposes, but in the light of the above passages from Aetius and Archigenes, Simpson's claim must be reduced to one of re-introduction " of a means of safely opening up the cavity of the cervix and body of the uterus, to such an extent as might enable us to introduce a finger into the uterine cavity,

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[1] A figure of the *vertigo albucasis* with which the womb was dilated, will be found on p. 208 of Professor Glaister's " Life of Smellie " (1894).

for the purpose of diagnosis and operation : [1] the " means " being the placing of sponge tents in the cervix.

But Simpson has not only been anticipated by the ancients, but also by modern surgeons, who have advocated sponge tents. Simpson [2] has himself shown that in the "Medico-Chirurgical Observations," of Henry van Roonhuyse, which were " Englished out of Dutch by a careful hand," in the year 1676, there is a chapter devoted to the consideration of the *Clausura Uteri* (p. 107), and in this chapter we find the following passage :

> In some women this neck (of the womb) is so hardened, tapering out and sunk down . . . that he was forced to enlarge it by means of the radix gentiana, medulla sambuci, or even by a prepared and dry sponge, having been first moistened in melted white wax and squeezed in a press to make of it convenient pessaries according to the exigency of the case, by which means the neck of the womb can be disclosed and widened.

In a work called the " Marrow of Chirurgery," published in the latter part of the seventeenth century by Mr. Cook, of Warwick, a work containing notices on obstetric and surgical practice, the author treats in one of his chapters of closure of the inner orifice of the womb, and recommends it to be enlarged when necessary by gentian root or a prepared sponge, and afterwards by the introduction of hollow instruments of silver, ivory, or horn, and he adds that these means are better than incision. Simpson came to the decision that Cook had not tried the different methods he refers to, but had merely copied a passage from Van Roonhuyse.

But even before Henry van Roonhuyse published his work,[3] Roderic de Castro,[4] of Portugal, published a book, in which he advocated the dilation of the cervical canal by expansile substances, such as the roots of gentian, aristolochia, bryonia, and cyclamen.

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[1] " The Obstetric Memoirs of James Y. Simpson," Priestley & Storrer, Edinburgh, 1855, vol. i. p. 125.

[2] " Clinical Lectures on the Diseases of Women," Simpson, Edinburgh, 1872, vol. iii. p. 246.

[3] " Geneesen heelkonslige aanmerkingen," Amsterdam, 1672.

[4] " De universa muliebrium medicina," Hamburg, 1603.

The laminaria tent was first introduced by Sloan in 1862.[1]

Dilation by means of straight metallic sounds of different degrees of thickness was introduced by Macintosh, a teacher in Edinburgh during the first quarter of the nineteenth century, for the treatment of cases of dysmenorrhœa due to a contracted cervix; while for the rapid forcible dilation of the cervix expanding instruments have been devised by Osiander, Carus, Aveling, Busch, Mende, Leblanc, Hunter, Nott, Atlee, Priestly, Ellinger, Miller, Wilson, Schultze, Ball, Palmer, Goodell.[2]

Ellinger was the first to recommend a two-bladed dilator for gynæcological purposes.

At the November (1879) meeting of the Obstetrical Society,[3] Tait for the first time showed his apparatus for dilating the uterine canal by continuous elastic pressure.

He says in another place: [4]

> The idea of employing continuous elastic pressure for dilating the uterus arose out of the success that I had in driving back an inverted uterus by the same method. I had long been dissatisfied with the risky methods of dilating previously in use by means of sponge and other tents. It never was very effectual and always dangerous, on account of the decomposition of the sponge, or the violent and uncontrollable force exercised by the sea-tangle or tupelo-wood. The methods employed for dilation by Hegar and others by the use of graduated ebonite stems, seems to me just as dangerous as the use of the tangle tent, and certainly is a piece of work so arduous for the surgeon, that I for one could not find time to use it.

This is Tait's description of his apparatus:

> A waist-belt is buckled round the patient, to which two tags of about six inches long are fastened fore and aft, and upon these are a series of hooks by which the tension of the elastic threads may be varied. The instrument itself consists of a straight stem; and I may say here, as I have said before, that I prefer all my instruments to be straight, so that I easily can tell in what direction they are going. The moment a curve is introduced, unless that curve is exposed to the eye,

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[1] *Glasgow Med. Jour.*, Oct. 1862.
[2] Wood's "Cyclopædia of Obstetrics and Gynæcology," vol. v. p. 110.
[3] *Trans. Obstet. Soc.*, London, vol. xxi. p. 291.
[4] "Diseases of Women and Abdominal Surgery," 1889, p. 108.

it is difficult to correctly decide where it is extending its influence. At one end of the stem there is a screw collar, and at the other end there are three holes in which are fastened the elastic threads, by which pressure is caused. To the screw collar are fixed in their turn, two, three, or four plugs, varying in size so that the point of No. 2 is a section of the middle of No. 1, and correspondingly the point of No. 3 is a section of the middle of No. 2.

No. 1 plug is pointed and is supplied with a collar, so that it may not pass into the uterus to a dangerous extent, and at one point this collar is cut off so that a finger may pass beyond it, in order that the operator may decide the amount and rate of dilation. No. 1 plug having been screwed on the shaft, the point is placed within the cervix, and the elastic threads fastened above the hooks, so as to secure the retention of the plug firmly within the cervix, but not to give pain. The nurse is then directed to tighten the threads by raising them two or three hooks at the end of a few hours, caution always being given that the patient is not to be hurt. If sufficient pressure is used for dilation and not more than that, the process can be carried on from beginning to end without any suffering at all, and under such circumstances it usually occupies a period of from twenty-four to thirty-six hours. But dilation may be made completely in six hours, if there is need of it, and the pain may easily be subdued by hypodermic injection of morphia. No. 1 plug is first driven in up to its collar, then removed, and No. 2 takes its place, and is in turn driven up on to its base, and then No. 3 if necessary. . . . For most operative purposes I find No. 2 plug gives all the dilation that is wanted ; but if it is necessary to explore the uterus by the finger, then No. 3 has to be employed.[1]

Tait's dilators have never become popular, and now are probably almost forgotten. They were difficult to adjust, they certainly caused pain, if not in Tait's hands, in those who tried to use them, and they were slow in action, and the medical man who employed them had to return and change the dilators from time to time. Another reason why such instruments fall into disuse is that they are not required when we know how to prepare and use laminaria tents, have Hegar's dilators, and one of the numerous expanding dilators, such as that of Goodsell or Ellinger ; Tait's

[1] To the *Lancet*, November 1, 1879, Tait contributes a paper " Twelve Cases in which Dilation of the Cervix Uteri was accomplished by Continuous Elastic Pressure."

instrument can do nothing that cannot be done by these means when used separately or in combination.

Tait, however, was quite satisfied with his dilators, and in 1889, ten years after he had brought them forward, he was able to write:

> For many purposes such as relief of stricture, straightening the uterus, or bad cases of flexion, or premature labour, this instrument is of incalculable value, and has now in my hands entirely superseded every other method of dilating the uterine canal. It is so absolutely free from risk, that I can now speak of many hundreds of cases in which I have employed them without even in a single instance having any anxiety arising from its use.

Hæmatosalpinx, Hydrosalpinx and Pyosalpinx. The Staffordshire Knot. Archæology

We now come to Tait's epoch-making operations on the Fallopian tubes. If we were to single out the one subject upon which Tait's future permanent fame will rest, we would not hesitate for a moment in selecting his original work in establishing the surgery of the Fallopian tubes as the greatest achievement of his life. Nothing that he ever did gave rise to so much criticism and opposition. Even the very existence of diseased appendages was denied, and his opponents did not hesitate to use terms about Tait which left their hearers in no doubt that they looked upon the provincial surgeon as nothing more than a liar.

The first case of tubal disease that Tait operated on was in 1877, and it was certainly a very remarkable case. The patient, aged thirty-eight years, was sent to him by Dr. Freer, of Stourbridge. In 1876 she had had some ill-defined illness, during which she had had some obscure pelvic pains, accompanied by fever. After this each menstrual period was painful, and in February, 1877, on being examined, a pelvic tumour was discovered. It was pear-shaped, movable and attached to the left corner of the uterus. Tait diagnosed a cyst of the parovarium, and advised operation. He saw her again in June, when her symptoms had become acute; her temperature was 101° F., pulse 130, and her evening

temperature rose to 103·2° F., accompanied by excessive pain over the whole abdomen.

Tait performed a section: he found a tumour which was evidently not ovarian, but which appeared to be a tubal pregnancy. He passed the trocar in and evacuated a quantity of thick dark brown fluid, having the peculiar smell of menstrual fluid.

> After the cyst was emptied, I passed my finger through the hole made by the trocar, and to my amazement, I found that the cyst had contracted; moreover, as I kept my finger in the cavity, I distinctly felt it contracting around and grasping my finger. Passing the forefinger of my other hand into the vagina, I made out that what I had opened was, beyond doubt, the left Fallopian tube, and that I must have opened it close to its fimbriated extremity.

Tait washed out the cavity with weak carbolic lotion, a drainage tube was fixed in, and kept in for three weeks; the drainage tube was then removed, and the patient recovered.

This first case Tait has described as a hæmatosalpinx, and his next tubal case was a hydrosalpinx.

The patient was placed under his care by Walker Williams of Birmingham. She was aged twenty-eight years, had been married, "and it is more than probable that gonorrhœa had been communicated to her about five years before I saw her." From that date she had had much pain at the menstrual period. When Tait examined her, he discovered

> a small cystic tumour behind, and to the right of the uterus, freely movable, but very painful when moved. I advised its removal, and this I undertook on May 23, 1879. I found it to consist of the right Fallopian tube distended by about a pint of clear fluid. The infundibulum was glued on to the right ovary, and the uterus part of the tube was distended like a tortuous sausage, the greater part of the cyst being made from the outer half of the tube. I removed the tube and left the ovary. She made an easy recovery, and is now in robust and perfect health, and has married again.

A few months later, Tait performed the third operation, and this time the disease was a pyosalpinx. The patient was twenty-six years of age, and the date of the operation

was October 11, 1879, and she recovered. This is shown by Tait's list,[1] but we cannot find any details of the operation.

In regard to priority in operative procedures on diseased tubes, the following is the order in which the operations were performed.

In November, 1876, Hegar successfully removed a tubal tumour which had been mistaken for an ovarian cyst, and in October, 1877, he removed a bi-lateral pyosalpinx. In November, 1877, he extirpated a bi-lateral hydrosalpinx, whose diagnosis had been based on the peculiar furrows and sausage shape of the tumour.[2]

Spencer Wells was the next operator, as the following passages from Alban Doran's work shows : [3]

> In 1877, I prepared for the College of Surgeons' Museum, a remarkable specimen presented by Sir Spencer Wells. It consisted of two Fallopian tubes dilated to such an extent as to constitute cystic tumours of considerable magnitude. The right weighed, on removal, 4 lbs. 11 ozs. ; the left, 1 lb. 6 ozs. Both contained a fluid like thin mucus. The left tumour was removed with a portion of the ovary ; several pedunculated cysts sprang from the broad ligaments.

In the same year, as we have mentioned above, Tait operated on a large hæmatosalpinx, and drained it ; this operation was performed in June, 1877.

There can be no dispute, therefore, about the priority of the operation, as regards the Fallopian tubes. Hegar operated on both pyosalpinx and hydrosalpinx before Tait did ; but as we shall show later on, it was Tait's work that first established the surgery of inflammatory diseases of the Fallopian tubes and pelvic abscess ; no one disputes this point now, though at one time Howard Kelly tried to make the world believe that Joseph Price was the man to whom the merit should be awarded. Thus he wrote in 1912 : " We can more readily appreciate the pre-eminent and original part Joseph Price has borne in the evolution of the treatment

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[1] " Diseases of the Ovaries," 4th edit., 1883, p. 333.

[2] " Handbook of General and Operative Gynæcology," Hegar and Kaltenbach, 1887, Wood, New York.

[3] " Clinical and Pathological Observations on Tumours of the Ovary, Fallopian Tube, and Broad Ligament," London, 1884, p. 169.

of pelvic inflammatory diseases in the eighties as he dictated the practice not only of this country, but to some extent of the entire world." [1] Now Joseph Price was a bold, rugged, brilliant surgeon with a love for truth, and he looked upon Tait as his master and his teacher, and some time before his death he wrote these words:

> The inventive genius of the profession of the period seems to be in the direction of "new methods." If there was less lying about results we would have less confusion and be better able to determine the value of any given surgical procedure, and out-patients would be greatly the gainers. While it is digressive, I regard it as an important and profitable question to ask ourselves: Have we advanced any in the last ten years in our relative position to the men who stand to us as pioneers, who gave us our first lessons, our advanced positions? If so, in what respect? Have what we have called our own advances, improved ways, lowered our mortality? In pelvic surgery Tait stood first—taught us the best we know. He has had no very close second. His disciples have not greatly improved upon his ways.

About this period Tait wrote to the *British Medical Journal* a letter on "Tait's Knot." [2]

This is the knot that is known as the "Staffordshire"; Tait gives the following account of it, and how he came to use it: [3]

> My own method of dealing with the pedicle is by a piece of silk thread, varying in thickness, according to the mass of the pedicle; for a thin pedicle I use thin silk, and for a thick pedicle correspondingly thick cord, because, of course, in the latter case the ligature must be pulled with much more force than in the former. The method in which I use the ligature is what I have termed the Staffordshire knot, as it is the badge of the county of Stafford; and the idea of its use occurred to me while travelling in a Staffordshire railway carriage, in which it was a conspicuous ornament. Mr. Mazzinghi, the learned custodian of the William Salt Library at Stafford, tells me that nothing is known definitely as to the origin of the badge, further than that it was granted by the College of Heralds to the county within living memory,

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[1] "A Cyclopedia of American Medical Biography," 1912, vol. i. p. xlviii.
[2] 1879, vol. ii. p. 813.
[3] "Diseases of Ovaries," 4th edit., p. 286.

and was copied from the arms of the old Stafford family at Maxstoke Castle. Such badges were used to distinguish the adherents of noble families, and their origin is generally altogether unknown. The legend in the present case is that rogues were at one time so numerous in Staffordshire that a knot had to be devised which would hang three at a time. If so, the original knot must have differed from its present representative, for the latter could only hang two. It is, however, its remarkable property in this direction which has led me to introduce its use in surgery.

The woodcut will show how the knot acts, and a very brief description will probably make clear enough how it is used. An ordinary handled needle, armed with a long piece of the silk required, is passed through the pedicle and then withdrawn, so as to leave a loop on the distant side. The loop is then drawn over the ovary or tumour, and one of the free ends drawn through it, so that one end is above, while the other is under the retracted loop. Both ends being seized in the hand they are drawn through the pedicle, against which the thumb and forefinger of the left hand are pressed as a fulcrum till complete constriction is made. A simple hitch is then made, as in the drawing, and tightened; and that is followed by another, as in ordinary ligature-tying. There is another and more complicated way of making the knot, by passing each end of the thread round the corresponding half of the pedicle, and crossing them within the loop in front, which is equally effective and which may be used in cases of large solid tumours.

This brings us now to the end of 1879, but the above recital does not constitute all Tait's activities, for we find that at the November meeting of the Medico-Chirurgical Society, after reading his paper on Cholecystotomy, he proceeded to bring forward *A Second Successful Case of Gastrotomy in Extra-uterine Pregnancy*. This second case was intended particularly to emphasise the propriety of operating by median abdominal section, and the necessity of leaving the placenta untouched.

Tait also records some months earlier in the year the fact that he had been successful in operating on a case of a "Parovarian Cyst which had been gangrenous from rotation and strangulation of the pedicle." [1]

In spite of his many operations, his papers prepared for

[1] The *Lancet*, January 28, 1879.

medical societies, his increasing work on the Health Committee of the Birmingham Town Council, he yet found time to organise the Birmingham Natural History Society, and to publish an interesting paper on the minute structure of Pitcher Plants.

He was always interesting himself in archæology, and the "Orientation of Churches," "Prehistoric Fortification," "Monumental Brasses," and "Britain during the Stone Age," were some of the subjects that engaged his attention and drew interesting papers from his pen.

XIV

Hepatotomy for Hydatid Cyst

1880

On August 16, 1880, Tait performed his first hepatotomy. Dr. Pike of Malvern asked him to see a patient, aged thirty-seven, a woman, unmarried, with the following history :

Between 1870 and 1872 she broke down in health, suffered from obscure symptoms of which she could give no very clear account, but which were referred by three practitioners to the spine.

In 1873 she had symptoms which were diagnosed as due to diaphragmatic pleurisy ; she was ill then for three months, and after that had bilious attacks, swollen legs, and dyspepsia.

In 1876 her friends noticed an alteration in her size. She had to have her dress let out, her breathing became interfered with, and an enlargement on the right side became apparent. This increased slowly till 1879, when it was evident that the whole of the right chest and abdomen were enormously increased in size, but it was not until February, 1880, that any attempt at a diagnosis was made, and the opinion then formed was that the patient had malignant disease of the liver.

In July she came under Dr. Pike's care, and he diagnosed the case as one of hydatid of the liver, and this diagnosis was confirmed early in August by Sir William Jenner, who advised the use of the aspirator.

Dr. Pike and Mr. Dawson, of Malvern, aspirated the tumour on August 11, and withdrew a few teaspoonfuls of clear fluid ; enough to establish the correct diagnosis of hydatid. When Tait saw the patient on August 15, 1880, he found her in such a condition that it was evident that death from suffocation and exhaustion was imminent if relief could not

be given : she was propped up in bed to relieve her breathing, and was vomiting incessantly. She was extremely emaciated, had a hay-like odour of breath, her features were pinched and her skin yellow ; in short, all the symptoms of extreme exhaustion. The hepatic dullness extended from the third rib down to the umbilicus, crossing the middle line to the left all the way for about two inches, and much more at the lower margin. The whole of the right side was occupied by a tumour ; no air was entering the right lung, and the left was greatly interfered with, while the heart was pushed over to the left. Below the right ribs, distinct fluctuation could be obtained over the tumour.

Tait, in recording the history of this case, says :

> Acting upon the principle that I have already advocated in previous communications to the Society of opening the abdomen in all cases of tumours where life was threatened, and of the malignity of which there was no certainty, I had no hesitation in proposing abdominal section in this case. Dr. Pike at once concurred in my proposal, and it was readily accepted by the patient and her friends.
>
> I therefore returned to Malvern the next day, August 16, and performed the following operation : Dr. Pike gave ether, and I was assisted by Mr. Dawson and Mr. Raffles Harmer. I made an incision four inches long, and about two inches to the right of the middle line, beginning at the edge of the ribs, and inclining slightly inwards towards the umbilicus. Having carefully secured all the bleeding points, I opened the peritoneum, and found that there was no adhesion of the liver to the wall, and that I had exposed healthy liver tissue. Into this I passed a large sized aspirator needle, and evacuated a few teaspoonfuls of clear serum as had been done before. Removing the needle, I passed my knife into its track, and made an opening large enough for my forefinger. I then found that the layer of liver-tissue was from an inch to three-fourths thick. I then fixed a pair of Koeberlé's catch-forceps on each of the margins in the wound in the liver, and asked my assistant (Mr. Raffles Harmer) gently to draw them up as I enlarged the incision. This I did to the extent of about three inches, and the moment I freed my finger, myriads of transparent globes of all sizes, from a pea to an orange, shot out, covering the table and floor, and which were afterwards picked off the floor all over the room. When the tension was relieved, I dug them out with a large silver gravy spoon, an instrument suggested to me by Dr. Pike, and this process took much more time than all the rest of the operation, and

during its performance, Mr. Harmer most carefully prevented any cyst entering the peritoneal cavity by keeping the flaps of the liver close against the abdominal wound. Finally, I perceived that my gravy spoon was causing some hæmorrhage from the inside of the cavity, which had no kind of lining membrane, and I had to leave a considerable quantity of cysts in the cavity. In the cut surface of the liver, two bleeding points gave me some anxiety, but I closed them temporarily with Koeberlé's forceps, and finally secured them in the stitches. This I applied by a common short needle and piece of silk in the continuous method, fastening the wound in the liver, through the whole thickness of the tissue, to the wound in the abdominal wall, so as effectually to close the peritoneal cavity ; I then fastened in a large glass drainage tube, eight inches long. The quantity of hydatid cysts evacuated is estimated by Dr. Pike to be about two gallons, and I think the amount is not exaggerated.

The patient did well; Tait saw her three days after the operation, when he found that the vomiting had ceased

and she was eating well, entirely free from pain and with the hepatic dullness contracted to almost normal limits. A large number of cysts had come through the tube daily with the discharge, which was faintly tinged with bile. Dr. Pike washed out the cavity twice a day with weak carbolised water. I saw her again on September 2 and found the wound healthy and that Dr. Pike had removed the stitches, and that the cavity held only about half a pint. Only one very small cyst had come away since my previous visit. I found also that she was gaining flesh rapidly, and eating well, her diet that day having included bacon, cheese, and porter.

The daily details of the case possess but little interest, beyond the fact that at the end of the first week a short (three-inch) wide glass tube replaced the long one, and in a fortnight more, a three-eights rubber tube was inserted instead of the glass.

Fragments of cysts continued to come away for about a month, and on October 17 there had been hardly any discharge at all for a fortnight, and nothing remained but a sinus.

Tait made a final note to this case : " The patient writes to me that she feels quite well, and is able to walk about alone, not quite eight weeks after the operation. She is now (1882) in perfect health, and is married."

The description of this operation, performed nearly forty years ago, is so like the description of an operation that might have been performed here in Australia at the present

day, that one must realise that Tait did in his first operation what we do here almost every week. He cut through the abdominal wall, the liver presented itself without adhesions, he stuck in his aspirator, and as the cyst was filled with "daughter cells" he got but a few teaspoonfuls of clear serum. The wonder is that it was *clear*, it is far more often like water with a little milk in it, or even like thin pus. Then he passed his knife into the track of the aspirator.

We know by experience in Australia, that a knife is a dangerous instrument when the layer of liver tissue between the surface and the cyst wall is from half an inch to three-quarters of an inch thick; a pair of forceps is a safer instrument to use to tear the liver with. Tait enlarged the incision for three inches. This was unnecessary, and is only done now when the cyst is quite on the surface of the liver, and an attempt is made to sew the sides of the cavity together, which is seldom possible or seldom necessary. His use of the silver spoon was a happy idea, and is one that we have all been glad to resort to, for it is quite impossible to get these daughter cysts out with forceps, and we can sympathise with Tait in his failure to entirely empty the hydatid of all its cysts. One wonders that he did not resort to his favourite plan of irrigation, for after the cyst wall has been attached to the abdominal wall, a strong stream of warm water is certainly the most effectual method of getting at the small grape-like cysts, which escape the spoon, the forceps, and every other means that can be employed to evacuate the endocyst of its contents. Tait fastened in a glass drainage-tube, and later on put in a rubber tube. Here in Australia, the rubber tube is still employed, and it is seldom that a hydatid cyst, that has been filled with daughter cysts, can be cured without drainage, and this often leads to suppuration.

The operation that we have recorded above, performed by Tait in 1880, may be claimed to be the operation that really established the practical operative treatment of hydatid cysts of the liver. Tait very soon performed other and similar operations, and in 1881, in a paper printed in the *Birmingham Medical Review* for October, 1881, he was

able to record four successful cases of hepatotomy. But while Tait brought the operation into the practical area of surgery in England, he was *not* the first to successfully operate on hydatid cysts of the liver.

Let us glance back and try and trace the evolution of the treatment of such cysts; but before doing so, let us very briefly draw attention to the pathological anatomy of such cysts.

In its normal state the echinococcus cyst lies imbedded in the liver, or some other organ; almost invariably it is completely enveloped in an adventitious sac, or capsule, which is not an integral part of the parasite, but is a morbid product of the organ, or tissue, in which the bladder-worm has taken up its abode; it is, nevertheless, a structure of great importance to the parasite, for through it, by a process of diosmosis, the latter obtains its supply of pabulum from the blood of the host; its functional integrity, Davies Thomas used to remark, determines whether the parasite shall dwell in plenty, or die of inanition.[1]

The operative treatment of hydatid disease before Tait's day resolved itself into:

(*a*) Operations which destroyed without removing the parasite;

(*b*) Radical operations.

Under the first heading we find that the following plans have been practised:

(1) Operations of puncture or "tapping"—aspiratory or simple, by which the fluid contents of the bladder-worm were removed to a greater or lesser extent.

(2) Injection through a canula of various bodies, supposed to act as parasiticides.

(3) Destruction of the parasite by electricity.

Under the second heading we have:

Those operations which had as their object the extirpation of the entire bladder-worm with any daughter-cysts that

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[1] "Hydatid Disease," vol. ii., John Davies Thomas, edited by A. A. Lendon, 1894, p. 136. This is the finest practical contribution on the subject of hydatid disease.

might be present ; such may be regarded as radical operations. To this class belong cases in which the parasite is removed by artificial opening into its sac, procured by :

(*a*) Caustics.
(*b*) Canule-à-demeure.
(*c*) Incision into sac.

Among the ancient writers, Hippocrates, Aretæus, Galen, and Rhases mentioned hydatids, and in a passage in Aretæus, where he is referring to ascites, he makes mention of the oldest method of treating hydatids, in the following words :

> This other form of dropsy is known ; small and numerous bladders full of fluid are contained in the place where ascites is found. But they also float in a copious fluid, of which this is a proof ; for if you perforate the abdomen so as to evacuate the fluid, after a small discharge of the fluid a bladder within will block up the passage, but if you push the instrument farther in, the discharge will be renewed. It is said, however, that in certain cases, such bladders have come out by the bowels.[1]

In the seventeenth century, Mayley and Dodard attempted to bring about adhesions between the surfaces of the peritoneum by the application of certain pastes that were first applied to the skin and later to the deeper tissues. Récamier, in particular, was responsible for this method : he used caustic potash, while Demarquay employed " Vienna Paste " or chloride of zinc.

Evacuation of the cystic contents by puncture was the favourite method of treatment, and Hulke, Borgherini, Mosler, and Peiper showed that after evacuation of the fluid the cyst collapsed and a cure resulted. Hulke and Savory proposed to remove only a small quantity of fluid with a fine trocar ; this, however, did not prove successful. Sibson then advocated the puncture with a fine canula, to which he attached a rubber tube, and allowed the cyst to drain slowly ; this plan was more successful.

After this, puncture and aspiration was tried. Budd was the first to advise the employment of a suction apparatus, and Dieulafoy and Potain both invented suction apparatus.

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[1] Stewart McKay, " History of Ancient Gynæcology," p. 129.

As early as 1851, Boinet injected tincture of iodine, and was followed by Velpeau, Richard, Weber, Aran, Demarquay, Chassaignac, and Schroetter; a few successes were recorded, but as some deaths were also reported the plan was abandoned.

After this, Mesward was the first to try an injection of solution of corrosive sublimate, and this gave promise of good results.

The next method of treatment was put into practice by Owen Rees, and later by Boinet and Verneuil. A trocar was inserted and allowed to remain for several days and then withdrawn; after this a drainage tube, or a double catheter, was inserted, and the hydatid cyst was drained until all the fluid was withdrawn.

Simon inserted two trocars: these were left in for a week; then a cut was made into the cyst between the trocars and the cyst was drained.

Before Lindemann performed his classical operation, Récamier and Begin had attempted to treat the cyst by making an incision through the abdominal walls and the peritoneum; tampons were then inserted and left for a week, and when adhesions had formed the cyst was opened.

Volkman found this method satisfactory, and Langenbuch collected forty-eight cases that had been so cured.

Now we come to Lindemann's operation. Davies Thomas has shown that Schlegtendal [1] has established the claims of Lindemann as the originator of this mode of treatment of hydatid cysts associated with his name in Germany. The first operation of this kind was performed by Lindemann in 1871, but the earliest account of the method was not published until 1879 by Kirchner.[2]

The history of Lindemann's first case is briefly this: The patient had a tumour of the liver, as regards the nature of which Lindemann differed from Strumeyer. An incision was made into the sac, and the opening was attached to the parietal incision by deep stitches, and the patient made a

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[1] "Die Lindemannsche Einzeitige Operation der Echinococcen der Bauchorgane," *Archiv. für Klinische Chirurgie*, 1886.

[2] "Ein Beitrag zur Operation der Echinococcen der Organe der Bauchhöhle," *Inaug. Dissert*, Berlin, 1879.

good recovery. Schmidt had even before Lindemann tried this method, but had failed.

Thomas says :

> Although this operation was apparently not premeditated in the form in which it was performed, yet the lesson taught by it was not lost, and in future Lindemann incised the parietis, exposed the sac, opened it, and stitched it to the abdominal wound, all under antiseptic precautions ; and thus he initiated what is without doubt the most effective and safe form of radical treatment.

As far as Tait was concerned he says that he knew nothing of Lindemann's operation. Knowsley Thornton delivered a lecture at the Harveian Society in 1896, and he stated that :

> Lindemann, in 1871, first brought hepatotomy for hydatids within the range of practice, stumbling into it, a process which has been so fertile in great advances, not only in surgery but in many scientific discoveries. In 1873,[1] Tait deliberately followed his lead, and in a succession of cases established the operation.

Tait replies to these remarks [2] and says :

> I am grateful to Mr. Knowsley Thornton for his kind recognition of my contribution to the establishment of "hepatotomy," but I wish it distinctly to be understood that whilst it may be said that I "followed Lindemann's lead," I did so quite unconsciously, and only as a matter of date. I did not know what Lindemann had done before me ; I am ashamed to say I do not know now, for Mr. Thornton's allusion to him and his work is the first I have seen.

Two years prior to the publication of Kirchner's work, Sänger published a method of operating which differed but little from Lindemann's plan.[3] The abdominal wall was incised down to the cyst, which was at once secured by stitches to the lips of the external wound, and then incised. Landau [4] operated in almost similar manner to Sänger, first

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[1] This date is a mistake, it should have been 1880.

[2] *Brit. Med. Jour.*, March 7, 1896, p. 629.

[3] "Zur Operativen Behandlung der Abscesse und Hydatider der Leber," *Berl. Klin. Wochensch.*, 1877.

[4] "Zur operativen Behandlung der Echinococcen in der Bauchhöhle," *Berl. Klin. Wochensch.*, 1880.

evacuating the cyst by puncture or aspiration, or through a thick trocar.

Tait, in his paper in the *Birmingham Medical Review* (October, 1881), does not make any mention of the operations performed by Lindemann, Sänger, or Landau. But this does not alter the question of priority, which stands thus: Lindemann first operated by the method now generally adopted for hydatid cysts of the liver; Sänger performed almost the same operation and published his account first, while Landau published his method after Lindemann and Sänger. Lastly, Tait established the operation as a recognised surgical procedure among English surgeons on the publication of his cases in 1881.

From the time when Tait published his first cases in 1881, and showed how hydatid cysts of the liver could be operated on successfully by opening the cyst and sewing the edge of the adventitious sac in the liver to the abdominal incision, and then inserting a drainage tube, the procedure has been looked upon as the classical and safest plan for dealing with liver cysts. But it was only natural that surgeons should attempt other plans, and as some of these have been successful, they have now taken the place, in selected cases, of the Lindemann-Tait method. With the advance of surgery one aim was always in view, to adopt a technique that would make the operation procedure complete: consequently operators have tried to abolish the drainage tube. This was first attempted by Knowsley Thornton, in 1883, when dealing with liver cysts, and then by Bond, in 1891, when operating on cysts situated among the intestines. Others tried to excise the whole hydatid from the liver substance, and when the cysts have been small and superficial this plan has been found possible.

Where, however, the cyst was too large to be excised from the liver, rather than drain the adventitious sac, Posadas (1898) suggested that the mouth of the sac should be closed by suture and dropped back; while Delbet attempted a difficult technique by which the walls of the adventitious sac were brought together by suture and the incision into the liver carefully closed, and then the whole dropped back

without any attempt at drainage, the abdominal incision being completely closed. Hamilton, of Melbourne, has done many operations on this plan.

These plans briefly represent the change in technique since Tait's day: let us consider these advances in detail so as to complete the history of the technique adopted in regard to hydatid cysts, not only of the liver, but of the abdominal cavity and pelvis.

Knowsley Thornton,[1] in 1883, published an account of two operations that he had performed on hydatid cysts. The first case was operated on December 20, 1880, by Tait's method, and died. The second case is the one that we are concerned with: because of this single operation surgeons are pleased to dub a certain procedure as "Thornton's Operation." Let us see what Thornton did, he will speak for himself.

Thornton tells us that he examined a woman in 1881, and found an abdominal tumour, the diagnosis resting between a hydatid, or other hepatic cyst, and an ovarian cyst, the possibility of its being a fibro-cyst of the uterus also presented itself. He says:

> I made a five-inch incision in the median line, and exposed a red looking tumour, which we all thought must be uterine. I passed my hand into the pelvis and found a small uterus, closely connected by its fundus with the lower part of the cyst. . . . Passing my hand into the upper part of the abdomen, I separated some vascular parietal adhesions, and could find no line of demarcation between the cyst and the liver. I tapped the cyst, and clean watery fluid escaped. . . . I thoroughly cleaned out the cyst-cavity, sponging the walls well all over. In parts they were calcareous. No fluid had escaped into the general peritoneal cavity, and as the cyst was very adherent to uterus, intestines, etc., I decided not to attempt any general sponging, for fear of causing hæmorrhage. I sewed the whole opening in the cyst into the abdominal incision, and having taken care to thoroughly dry the sac, closed it up entirely without introducing a drain of any kind. . . . I did not disturb the dressings till the eleventh day, and then found the wound well healed.

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[1] *Med. Times and Gazette*, 1883, vol. i. p. 89.

In the *British Medical Journal*, April 11, 1891, Bond published some observations, "On the Treatment of Hydatid Disease by Incision and Evacuation of the Cyst without Drainage." The object of this paper was to bring forward a method "of incision and evacuation of the cyst contents without any subsequent drainage, or with only temporary drainage for a few hours after the operation." The case that Bond had to deal with was not an instance of hydatid cyst of the liver, but a case when the cyst was situated between the bladder and rectum, beneath the peritoneum; this had grown upwards, filling the pelvis, blocking the rectum and urethra, and reaching up to the level of the umbilicus, and resembled a full term pregnancy; the bladder was dragged up with it, and the fundus, though empty and compressed, reached to within two inches of the umbilicus. The cyst was opened and the endocyst was found curled up loose in the cavity together with much pus and daughter cyst; the cyst, it may be remarked, had been aspirated three weeks previously. The endocyst, pus and daughter cyst were removed and the cyst was drained. Bond then says:

> The important feature in the case, however, follows. On exploring the general peritoneal cavity, I found in a reflection of the peritoneum in the right iliac fossa a small cyst, the size of a tangerine orange. As I could not drain it, I incised the cyst and removed the elastic membranes [endocyst] and clear fluid. This being a growing cyst, I also scraped away some of the inflammatory adventitious cyst as well, immediately outside the elastic cyst proper; but this was, I think, unnecessary, and might lead to hæmorrhage. The remains of the cyst were then returned into the peritoneal cavity; no bad effects followed, and at a subsequent operation on the same patient no trace of the cyst could be found.

Five months later a second operation was performed on this patient for a cyst situated in the epigastric region, and to the operation performed on this cyst has been assigned the label "Bond's operation." This is what Bond says:

> I opened the abdomen by a median incision passing through the umbilicus. I then discovered three cysts, the large one referred to above, which lay beneath the transverse colon

> and omentum, and two others the size of cricket balls, one in the left renal region apparently in connection with the left mesocolon, and one quite movable at the lower part of the cavity, situated entirely in the omentum.

The cysts were incised, the endocysts were removed, and the cavity of each cyst wiped dry. Bond found that when he tried to draw the openings in the cysts up to the abdominal incision that a

> good deal of dragging on the abdominal contents occurred, so I simply closed the abdominal incision in the usual way, allowing two or three of the silk sutures to pass also through the cut edges of the cyst walls; between these two drainage tubes were inserted, one into the lower and one into the upper cyst; these were removed in sixteen hours, and the abdominal incision allowed to close thoroughly. Union occurred at once, with the exception of a small spot at the umbilicus, which continued to discharge. The temperature remained normal throughout, and in a month's time he was out of doors. Unfortunately, at this date, owing to some strain, or chill, or other cause, an attack of abdominal pain and violent vomiting occurred, which was followed by some discharge of pus from the incision, and Dr. Serres removed several pieces of laminated lining of the upper cyst. This ceased in a week's time, and the patient soon became again convalescent.

Posadas,[1] in 1898, proposed to close the incision in the liver after taking away the fluid and the endocyst, and he did not attach the adventitious cyst to the abdominal wall.

Delbet proposed, and carried into practice about 1898, the following procedure: the cyst was emptied, the endocyst was removed, then the walls of the adventitious cyst were brought together so as to obliterate the cavity, and the opening into the liver was not attached to the abdominal walls; the abdominal incision was closed without drainage.

These are the chief lines along which surgeons have travelled since Tait's first operation, in the endeavour to get away from the drainage tube.

Of the procedures mentioned above we consider that Thornton's plan is the one most worthy of adoption in suitable cases. This surgeon was one of the first in London to recognise and practise Listerian precautions, and no doubt this enabled him to succeed when others would have

[1] *Rev. de Chir.*, Par. xix. 374.

failed. He very rightly reasoned in his case, "that once thoroughly cleared of hydatids, the sac would not be a secreting one, as it had not suppurated, and if a little serum was effused into the cavity it would gradually reabsorb as the sac contracted." He had to take his chance at a bile duct opening into the cavity; this is not unusual.

An experience of a quarter of a century in operations on many hydatid cysts in various parts of the body has convinced us that the drainage tube is used much too often, and for the last ten years we have endeavoured to eliminate it as much as possible when operating on liver cysts. In all cases of endocysts when there are no signs of suppuration, after removing the fluid and the endocyst, we sew the adventitious cyst wall to the abdominal incision, the catgut passing through the peritoneum, the muscles, and the aponeurosis. The cyst is then swabbed with peroxide of hydrogen solution, and the sac is filled with plain gauze so as to dry out all the fluid. The abdominal incision is closed and strips of plaister are applied so as to exert as much compression on the abdominal walls as possible, in the hope that the cavity in the liver will be reduced.

If, at the outset of the operation, we find on introducing an aspirating needle that we get very little fluid, and that fluid is cloudy, we must take it for granted that daughter cysts are present, and it is then advisable to attach the walls of the adventitious cyst to the edges of the abdominal incision before the hydatid cyst is opened; by this procedure we lessen the risk of the escape of the cysts into the peritoneal cavity.

There is one point in the technique which we think is of much importance, in fact it is probably the key to success. We believe that the insertion of the drainage tube into the adventitious sac is the quickest way to lead to suppuration in the sac, and in order to avoid suppuration it is absolutely necessary as soon as the incision is made through the skin to sew, or fix by forceps, gauze to the edges of the incision; by this simple means we can hope to avoid the use of the drainage tube in clean cases, and avoid suppuration when the drainage tube is not used.

While the above technique is the one which it is desirable to attempt it is not so likely to succeed when the mother cyst is very large, and is full of daughter cysts, and in such a case great difficulty will be experienced in ridding the cavity of all the small cysts. Irrigation will help, and in order to do this thoroughly we have had a zinc box constructed about six feet long, two feet six inches wide, and six inches deep; the upper portion of this being crossed by five or six flat bars three inches wide, upon which the patient is placed after being lifted from the operating table. This irrigating box is brought into the theatre on a trolly and placed by the operation table, and the patient is lifted on to it; the chest and legs are then covered. This arrangement allows one to irrigate the cavity with gallons of fluid and there is no mess from the fluid that pours out of the cyst. After the irrigation is finished the cavity is dried by means of the gauze, which is then withdrawn.

In cases where suppuration prevails the drainage tube must be used; while in cases where there are calcareous patches in the walls of the adventitious sac the above method may be tried, but it is not so likely to succeed, as such cysts are always old and degenerated and of low vitality.

In those cases when the bile channels open into the cavity left after removing the endocyst, some means of escape for the bile must be adopted. Frequently the bile is poured out in such cases for weeks, and sometimes months, and consequently suppuration is not infrequent, though in some cases it would appear that this bile-stained fluid almost acts as an antiseptic and wards off infection.

As regards Delbet's operation, while it is possible in a small number of cases, it is quite impossible of execution in dealing with large cysts, and we do not consider that it is ever necessary to sew the sides of the walls of the adventitious sac together.

With regard to Bond's operation, we consider that all hydatids that grow in among the intestines should either be completely removed, or when that is not possible, the cyst should be opened, the contents removed, the walls of the adventitious cyst well washed with peroxide of hydrogen

and then returned to the peritoneal cavity. This, of course, applies to all clean cysts; if a cyst shows any sign of suppuration, then the incision in the cyst must be attached to the abdominal wound and a drainage tube should be sewn in, in a similar manner to some of the methods adopted in draining a gall-bladder. We have treated many cysts by Bond's method and always with success.

There are some few cases when the cyst can be excised completely from the liver, but they are rare; the only cases in which we have attempted to carry out this procedure were small cysts which hang down from the lower surface of the liver and the cyst is not buried to any extent in the liver substance. In some cases we have been able to excise all but the base of the cyst wall, the base is then rubbed with peroxide and fixed to the edges of the abdominal incision.

Note.—There are two terms very often used in connection with operations on hydatid cysts of the liver, these are *Marsupialisation* and *Capitonnage*. Moynihan says: "For the procedure of applying sutures so as to close the cavity by causing approximation of its opposing surfaces Delbet employs the term 'capitonnage' (the marsupialisation of the Australian surgeons)." This, however, is an error. By marsupialisation we mean the sewing of the edges of the adventitious sac to the edges of the abdominal incision, thus creating a pouch, somewhat like the pouch in which the marsupials carry their young. Capitonnage is the application of sutures so as to close the cavity of the adventitious sac by causing apposition of its opposing surfaces.

It is interesting to note that Moynihan ("Abdominal Operations"), the Mayos (Keen's "Surgery"), Stewart (Bryant & Buck), and Kehr (Bergmann, Bruns & Mikulicz's "System") never even mention Tait's name in connection with the methods adopted for the operative treatment of hydatid cysts of the liver! Well may Henrietta Huxley, in writing about her illustrious husband, remark: "Although a man by his works and personality shall have made his mark upon the age he lives in, yet when he has passed away and his influence with him, the next generation, and still more the succeeding one, will know little of his work"!

XV

Ovariotomy and Listerism

1880

Tait had now arrived at a stage in his career when his name was known, not only in Birmingham, but throughout England. His original work and his brilliant success were attracting attention on all sides. He never failed to read his important papers at the London Medical Societies, and we can now detect a note in his replies to his critics which shows us plainly that he no longer bowed to their authority; deference was no longer his policy; open defiance was the attitude that he assumed. He now began to dictate to his seniors; he was the teacher, they were to follow him in the future.

Knowsley Thornton was very antagonistic to him; he and Tait never missed an opportunity of crossing swords. Thornton was one of the ablest surgeons of the day, and his opinion was respected.

Spencer Wells was now no longer the model to be followed, and quite openly Tait objected and ridiculed his practice, and condemned him for having persistently adhered to the clamp in his ovariotomies. Tait now had his own following in the counties, and every month cases were sent to him in which he considered it necessary to remove the ovaries or tubes, either to check the hæmorrhage due to a myoma of the uterus, or to relieve sufferings due to pyosalpinx or other diseases of the Fallopian tubes.

During this year he relates how he was called to operate on a woman of very eminent social position, and he proposed to remove her ovaries in order to check a metrorrhagia that endangered her life. Speaking of the case many years after, he said that it was one of the most important operations that he had ever been called upon to perform, and he says: "I

had the courage of my convictions and I proceeded, as a last resource, to a step which, if I had regard to metropolitan opinions, I should not have attempted. Had the case been unsuccessful, the position of the patient was such that the proceeding would have been widely and I fear adversely criticised." [1]

Tait performed a section and removed the ovaries, which he found small and cystic, and he thus made the observation, which has since been confirmed on all sides, that with small cystic ovaries, we may get the most uncontrollable hæmorrhage from the uterus.[2]

This patient was of great importance to Tait, and as she was well known, was the means of procuring for him many patients of her own social position and wealth. The husband, too, was enthusiastic, and wrote to Tait: "It only remains for me to express our united gratitude for your skill and attention; for, humanly speaking, I shall always look upon you as her saviour."

On February 10, 1880, Tait read a paper before the Royal Medical and Chirurgical Society on "The Antiseptic Theory tested by the Statistics of One Hundred Cases of successful Ovariotomy."

Tait had more than one object in view in reading this paper. We saw that in 1878 he had published the result of his first fifty cases of ovariotomy, and that he had had the very unsatisfactory result of nineteen deaths: mortality, 38 per cent. Now he wishes to make it known to the metropolitan surgeons that in his second fifty cases, he lost but three cases: 6 per cent.; a truly remarkable and rapid improvement in results. But this was not his only object; his second aim was to openly announce his disbelief in the Listerism technique, not only when carried out in the various steps of the operation, but also as regards the dressing of the wound; and in order to make his position quite clear in regard to the latter he wrote a letter to the *Lancet* that we shall notice presently.

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[1] "Diseases of Women and Abdominal Surgery," 1889, p. 426.
[2] Tait, "Diseases of Women and Abdominal Surgery," 1889, p. 424.

In the paper referred to above, Tait begins with the admission that the germ theory was substantiated, and that no process of putrefaction occurs except, as he puts it, by the admission of resting or swarm spores of living organisms associated with putrefactive change. He dwelt on the fact that the materials on which experiments had been made were dead, and that no corresponding putrefactive changes had resulted from the admission of germs to living matter, and he quotes Dr. Roberts, who stated that the hypodermic morphia syringe will inoculate sterilised solution of dead organic matter, but there are no putrefactive changes from it in the healthy body, and there is no evidence that the changes which occur in blood poisoning—even when of local origin—have any analogy with a putrefying dead infusion.

Tait then maintains that if the germ theorists were correct, no surgical operation would be successful without rigid antiseptic precautions, and a cut should be followed by septic poisoning, and surgery must long since have been an extinct art. The three factors, he held, on which the operating surgeon finds his success to depend are, the condition of the patient, the condition of the surroundings, and the nature and extent of the operation.

Tait proceeds to put forth the proposition that in minor operations germs have no influence, and that in major operations the condition of the patient to resist their influence was of the most importance. He ventured to say that one of the dangers of antiseptic surgery appeared to be that it was thought that many operations hitherto considered impossible might be undertaken safely under its influence, and that as a road to surgical success it placed the skilled and the inexperienced on a common level; but he feared there had been bitter lessons learned, showing that an antiseptic spray would not compensate for the want of manipulative dexterity.

Tait then works in the fact that in his second fifty ovariotomies he had lost only three cases, and goes on to say that mortality results indicate only the increase of skill, for a recovery after ovariotomy is the sum of a number of efficient details; but a death may be the failure of one detail only,

not perhaps under the control of the surgeon. In the three deaths that he had to record in his second series of fifty cases, two were due to details beyond his control, and had no relation to the antiseptic system, while the third, with full antiseptic details, was due to the irritative effects of thymol. In twenty-nine cases treated antiseptically there were two, and in twenty-one cases without antiseptic precautions, only one death.

Tait then remarks that Spencer Wells' statement, relating to the marked improvement in his results since he had used antiseptics, must be qualified by the fact that he had concurrently adopted the intra-peritoneal method of dealing with the pedicle—a method which had been very successful in the hands of Keith and of the author—and the statistics of which go to prove that little of its success could be attributed to antiseptic precautions, though Tait admitted that he was not prepared to say that they were entirely without result.

He then goes on to say that some of the details of the antiseptic method prevented the healing of wounds, and this was his uniform experience; he did not, however, attribute the delayed recovery met with in cases where antiseptics were used to Mr. Lister's processes, but he wished to show that the recovery of his patients was not facilitated by them. He had followed closely the published details of those who advocated the antiseptic system, and was at first strongly impressed by its influence; but after a large experience he began to suspect that he was attributing to it effects due to other causes, and his suspicions were confirmed by the facts given in the paper, which would cause him to modify his conduct in the dressings, etc., though he should continue to employ the antiseptic precautions during the operation, unless a wider experience decided that they also might be given up.

In the discussion that followed, the President, Erichsen, said that Mr. Tait's position seemed to be that while he admitted the germ theory, he challenged its application to surgery.

Spencer Wells said that he would not admit that it was

the intra-peritoneal method of treating the pedicle that had improved his (Wells') statistics; he maintained that it was the employment of Listerism, and he affirmed that the intra-peritoneal treatment of the pedicle was only possible with the use of antiseptics.

Mathews Duncan said that he could not agree with Tait, and he held that it was useless to speak of not applying antiseptics in surgery, and he considered that every one did this in some form or other.

Knowsley Thornton, MacCormac, and T. Smith all disagreed with Tait, who, in his reply, maintained that his position was unshaken. He still used the spray, and that alone. He used no antiseptic gauze whatever. He held firmly that the pulse curves were more important than those of temperature. His concluding remark was quite in the Tait style of the future, "the belief of some in Listerism was like that of some in the doctrine of election—none could understand or work it who were not fully possessed of it;" a remark which had more truth in it than he even suspected.

One may say that this paper was the opening of Tait's campaign against Listerism, and that from this time onwards he waged war against antiseptics and the followers of Listerism. We shall return to this subject later on, when we shall give a full exposition of Tait's views, merely noting here that a few months after reading the above-mentioned paper Tait wrote a letter to the *Lancet* [1] on "Lister's Antiseptic Dressing." The object of this letter was to call attention to Tait's view that the dressing of gauze and protective, upon which Lister and his disciples laid so much stress, were not only useless, but even positively hurtful by irritating the wound, and preventing speedy and complete union. He stated that he now had fifteen ovariotomies, two abdominal explorations, and four excisions of the breast, in which he had used plain Gamgee dry gauze dressings without any antiseptic preparations, and "not only have all the cases recovered, but they have not given me five minutes' anxiety

[1] The *Lancet*, April 24, 1880.

about their recovery. It is, therefore, to me conclusively proved that our improved results in abdominal surgery do not depend on Mr. Lister's dressings, at any rate ; and now I intend to exclude the other details one by one, and place the results on record, whatever they may be."

Tait practically followed the above plan of dressing the wound in all his future sections ; adding, however, powdered boracic acid in after years, not because of its antiseptic powers, but because he considered that it kept the skin surface drier more effectually than gauze did when used alone.

This letter is of interest in connection with the history of the evolution of the *aseptic technique*. Tait being dissatisfied with Lister's dressings impregnated with antiseptics, determined to try plain gauze and see the result. A series of cases in a few months showed him that plain gauze was sufficient, and from that moment he cast aside the moist impregnated dressings, and we know now that he was quite right in doing so, although the bad effects that he imagined arose from Lister's dressings were not probably due to the dressings, but to those who applied them to a wound in which defective technique had caused infection during the operation.

Every new step that Tait takes from this time onward is of interest, for he was now the chief explorer in the new realms of surgery. He had cast aside the clamp ; he had adopted intra-peritoneal ligature of the stump in removing ovaries and tubes ; he still used the spray and immersed his instruments in carbolic lotion, for he had not yet reached the stage where he felt safe in abandoning these details ; nevertheless he determined, as he said, to drop the details one by one, and he did this by first of all casting aside moist Listerism dressings. How he further abandoned the spray, and the other " rites," as Treves once called the antiseptic precautions of some surgeons, we shall see later on.

When we read Tait's communication at this period, we cannot fail to see that his success in ovariotomy, which he, no doubt, regarded very much in the same light as a bacteriologist would some test experience by which he was able to

check his other work, was giving him an assurance in his work which he had never had before, and so he ventures to make a display of such work, and to draw conclusions, always, if possible, to his own glorification, and to the detriment of Wells, Thornton, and Lister. For instance, he opened fire on metropolitan surgery and antiseptics in a letter contributed to the *Medical Times and Gazette* (January 26, 1880), on the "Listerian Method of Ovariotomy." He begins by saying that in a paper published by Knowsley Thornton he tells us that his mortality in ovariotomy, performed with full Listerian precautions, is 10·76 per cent., and that if he had known this when his (Tait's) paper was being discussed at the Medico-Chirurgical Society, he would have said more in his reply than he did. He then continues:

> In this discussion, the position I held was, that the great success in recent ovariotomy practice is to be put down to the complete adoption of the intra-peritoneal treatment of the pedicle, and to increased experience on the part of the operators, and that so far as we have got, there exists no physical proof whatever that the Listerian precautions have contributed in any way to this increased success. In my own practice, dressings have done a great deal of mischief, and, therefore, I have given them up.
>
> Mr. Knowsley Thornton's figures give full support to my position, and so do all the statistics I can find. Let me display a summary of them.

Clamp Cases without the Antiseptic Precautions.

	No. of cases.	Mortality per cent.
Dr. Keith	47	19·2
Mr. Spencer Wells	627	20·73
Mr. Thornton	6	50·0
Mr. Lawson Tait	37	25·0
Clamp Cases with full Antiseptic Precautions.		
Mr. Lawson Tait	26	26·9
Intra-peritoneal Cases, Non-Antiseptic.		
Dr. Keith	50	8·0
Mr. Thornton	30	16·6
Mr. Lawson Tait	17	6·6
Intra-peritoneal Cases, Antiseptic.		
Dr. Keith	50	6·0
Mr. Thornton	150	10·76
Mr. Lawson Tait	56	4·35

Now it seems to me that it is utterly impossible to look at these figures, gathered from the practice of four different surgeons—the experience of all being large, and in one case (that of Mr. Spencer Wells) most exceptionally large—without seeing that what I have repeatedly said is correct: that the normal mortality of the clamp is about 20 per cent.; and this is not in the least affected by antiseptics.

The moment the pedicle is treated by any of the intra-peritoneal methods the mortality drops to 10 per cent. again, quite independently of antiseptics; and then the mortality is further steadily reduced by growing experience on the part of the operator, as is most conclusively shown by the case of Dr. Keith, where mortality slowly fell to something like 6 per cent. before he used the antiseptic method at all.

I have already published my experience as having decided me against Mr. Lister's dressings. Mr. Thornton now tells us that there is risk in the spray. We all suspected this, but Mr. Thornton has been the first to experience it, or, at least, to form a decided opinion concerning it. I think if I were to have two deaths from the spray, I should give it up. At present I have seen no harm from it.

Now if Tait fondly imagined that he could attack a man like Knowsley Thornton with impunity, he was soon very disagreeably surprised, for in the July 10 number of the *Medical Times* Thornton replies to him, and replies in a way which not only hurt Tait, if we are to judge by his reply at a later date, but also taught him a lesson in accuracy in figures which he was not likely to forget.

Thornton's letter was entitled "The Listerism Method in Ovariotomy," and he opens with the following sentence:

The general testimony in favour of the improved results obtained in ovariotomy by the application of Lister's method to this operation is so strong that I think it will require some more powerful and conclusive evidence than that contained in either of the two recent papers [1] by Mr. Lawson Tait, to shake the faith of the profession in antiseptic ovariotomy; for if these papers are carefully examined, and the statistics they contain are compared with Mr. Tait's own work before he adopted Lister's method, so far from running contrary to

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1 "A Second Series of Fifty Cases of Ovariotomy, with three Deaths," by Lawson Tait, read before the Birmingham and Midland Branch, January 8, 1880. "The Listerian Method in Ovariotomy," *Med. Times and Gaz.*, January 26, 1880.

> the general verdict, they really afford most important support to the views of those who credit Listerism with a larger share of the present low mortality after ovariotomy.

Thornton then points out that Tait's first series of ovariotomy operations was performed without antiseptics, and the result was a mortality of 38 per cent. "A higher mortality than that of any British surgeon who has published his statistics."

We can well imagine how this sentence would incense Tait, because he knew that there was no reply to it. It was true!

Then Thornton goes on to point out that Tait gives the result of his second series, and with the exception of the first four cases of the fifty, the rest were antiseptic technique cases, and the result was a mortality of only 6 per cent., therefore "the evidence of facts is all in favour of the improvement being due to, as it is coincident with, the employment of Lister's method."

Thornton continues very fairly:

> Mr. Tait is very fond of comparing his results with those of other surgeons; but surely he cannot suppose seriously that any comparison can exist between his series of 50 cases, operated on in 1878 and 1879, and the corresponding series of Mr. Spencer Wells and Dr. Keith operated on many years ago, under very different surgical conditions, and when every step of the operation of ovariotomy had still to be learnt and worked out. Such comparisons are most unfair.

Thornton then points out where Tait's figure are wrong. Tait had given the following figures:—

"Tait 56 cases, 3 deaths, mortality, 4·35."

Thornton points out that the mortality should have been 5·35, and he goes on to point out where Tait's figures are again wrong, and says: "Are such impossible and imperfect statistics as these to be put in comparison with those of surgeons who publish their results in full and detailed tables, such as are from time to time given to the profession by Mr. Spencer Wells, Dr. Keith, and myself?"

In concluding, Thornton taunts Tait with the fact that in his paper he had spoken of his *large* experience when he

had in reality operated on 100 cases only : " I may be allowed, however, to express my astonishment that Mr. Tait, with his large experience," etc.

Tait, in the July 24 number of the *Medical Times*, replied to Thornton, and in opening has to acknowledge that his figures were wrong : " I entrusted the statement of the figures to a clerk, and his knowledge of decimals has proved defective. I have worked these out myself and find that the amended display is more in my favour than the mistaken one ; " then quite unabashed, he makes the following statement : " The only mistake of any importance is where the mortality in my last series is made to be 4·36 per cent. instead of what it ought to have been, 3·57 per cent. (two deaths in fifty-six cases), or 3·3 per cent. as it would now be (two deaths in sixty)." Tait does not take the trouble to explain why it should be *two* deaths, when his paper distinctly said that it was *three* deaths, and so in Thornton's reply he notices this ; so Tait replies in a letter saying : " Of the three deaths in the fifty cases he quotes, the first was a clamp case, and therefore is out of the present discussion.

" The sixty cases with two deaths are intra-peritoneal cases, and amongst them there have been only two deaths."

Tait concludes his letter thus : " His sneer at Dr. Bantock will doubtless be answered by that gentleman himself ; but I think that Mr. Thornton will hardly venture to sneer at Dr. Keith's letter in the *British Medical Journal* of to-day, where my conclusions are practically repeated."

In Keith's letter [1] there is a passage which, as Tait said, supported his views ; it is :

> But, after all, what most concerns us now is, by whose method may ovariotomy be performed with the least risk to the patient ? Surely the one that gave us a death-rate of less than eight per cent., long before antiseptics were heard of, is the one to trust to now—such, at least, is my experience. The cautery alone gave the best results of all the methods before. It gave better results fifteen years ago than any other method can yet show with antiseptics. Helped by drainage—for where would the antiseptic system be without

[1] " The History of Ovariotomy," *Brit. Med. Jour.*, July 31, 1880.

drainage ?—it gives the best of all results with them. Ninety-eight of my last hundred cautery operations have recovered, and in one of the two fatal cases the tumour was malignant with cancerous masses in the pelvis, practically an incompleted operation.

Tait in the August 31 number of the *British Medical Journal*, makes use of Keith's letter as an excuse to announce that he has now performed ovariotomy 73 times since he abandoned the clamp with only two deaths, and says :

> The question as to whether the cautery or the ligature forms the better of the two intra-peritoneal methods of dealing with the pedicle, yet remains to be settled. So far I do not see any advantage for either plan over the other.
>
> Dr. Keith asks, very naturally, what would "antiseptics (Listerism) be without drainage ?" I cannot answer him, for I do not think the question worth further discussion. I never have drained one of my cases of ovariotomy, and I do not think I ever shall ; but I have abandoned Listerism, as a source of more danger than advantage ; and yet I am getting now success as great as Dr. Keith's.

XVI

The Influence of the Intra-Peritoneal Treatment of the Pedicle of Ovarian Cysts on the Progress of Abdominal Surgery

1880

We have now come to the year 1880, and we propose to pause here and put forth the proposition that once Tait realised the importance of the intra-peritoneal treatment of the pedicle of ovarian cysts, he held the key by which he opened the gate that admitted us to the path along which abdominal surgery has made its triumphal march. We maintain that from the moment that Tait realised the importance of the intra-peritoneal treatment of the pedicle, from that moment he became the real founder of the surgery of the abdomen. We also maintain that his future success hinged upon this piece of particular knowledge; and lastly, that Tait's operative treatment of the diseases of the Fallopian tubes blazed a path which led out of the pelvis to every diseased organ in the abdomen that has been attacked by the surgeon. It was only natural that this surgical attack should come in due course; it was the outcome of experience. One organ after another was viewed with apprehension, but when the attack was successful others attempted to do more; and so every diseased organ in the abdomen became a pathological citadel for the venturesome surgeon to attempt to conquer.

This claim that we make for Tait may be resented by some, but a close study of operative procedures, in connection with the abdomen organs, shows us that they originated only after surgeons were convinced that the peritoneal cavity was not a place that need be held in such great reverence, and it was Tait who showed that, instead of the peritoneal cavity being a place that could not be touched without

great risk, it was in reality a place where the most prolonged manipulation could be practised with comparative impunity. And in spite of all he said and wrote against antiseptics, he had learned his lesson from Lister; and while he knew that all the complicated detail laid down by Lister was not really necessary for success, he had the sense to know, as his great master Syme had known, that cleanliness, whether attained by antiseptics, or by any other means, was the password that admitted every surgeon into the field of safety and success in whatever branch of surgery he worked.

But let us come nearer to the point that we are directing attention to. We know, from our frequent conversations with Tait, that he was stung to the quick by the awful results of his first fifty cases of ovariotomy. He had had nineteen deaths with a mortality of 38 per cent. Spencer Wells had a standing mortality of 25 per cent. Tait had at the outset of his career the most excellent results in ovariotomy, but after a time he had adopted Wells' method of treating the pedicle, and that was his undoing.

Faced with the great mortality of this, his first series of abdominal operations, he determined to abandon the clamp. He had conscientiously adopted Listerism, and carried it out in its fullest details, according to his lights, but his first fifty cases convinced him that there was something wrong in his technique, so with a courage and prescience which Spencer Wells did not possess, he determined to cast aside the clamp, to ligature the pedicle, and return it to the peritoneal cavity.

We hold that Tait's determination to abandon the clamp and adopt the ligature had the all-important influence on abdominal surgery; for no sooner had he adopted the ligature than his results improved, and as he became day after day more successful, he realised where he had failed, and when he became more sure of his ground, he determined to strike out and attack other organs that a little while before had appeared beyond the sphere of successful surgery, if judged by the results obtained at that fatal period in the history of surgery.

No sooner was he fairly launched upon his new task, than he began a campaign against Listerism. We shall deal with Tait's views on this subject in a later chapter. We shall here merely remark that we are not in sympathy with him, because, without using any more delicate expression, he was too pig-headed to realise that while the details laid down by Lister were often not necessary, and that every year of experience made a change in detail a necessity, that Lister had given the surgical world something so good that if Tait had given it honest consideration it would have shown him that Lister's proposals, backed up as they were by ten years of marked success, were indeed the keynotes to successful surgery. Tait never wanted to follow, he always wanted to lead, and so he started a technique in opposition to Lister.

Tait maintained that all that was necessary was to be *clean*, and that his master, Syme, was successful because he had been as clean as any human being could be about his person and his hands ; but Tait would not see that there was ordinary cleanliness and *super-cleanliness*, and that the latter, amounting to an absence of germs on the hands, the ligatures, and the instruments, could only be obtained by sterilisation. Lister had pointed out how difficult it was to be surgically clean, and had invoked the aid of antiseptics to help him to attain the end that he had in view. Tait said that he could be as clean without such aids, and ridiculed the teachings of the careful and accurate Lister. Yet, with all his diatribes against this great man, there no doubt had sunk into Tait's unwilling brain the cardinal fact which Lister had impressed on the whole surgical world, and that was, that the secret of success was to exclude septic matter from all surgical wounds. Lister adopted one method, Tait tried another, but he would never admit that Lister was his teacher.

Tait found by experience that the peritoneal cavity was a place in which one could take liberties up to a point, and that neither the spray nor the elaborate precautions laid down by Lister were necessary. So he began a plan of his own, which he imagined had nothing to do with Listerism,

and this led to the evolution of the *aseptic* plan of surgical technique, which is the final and most perfect form of Listerism.

We, therefore, make the following assertion, that when Tait realised that the intra-peritoneal treatment of the pedicle was the key to successful ovariotomy, he saw at once that the removal of other organs in the abdominal cavity was nothing more than a mere extension of the principles followed in ovariotomy, that, provided a diseased part could be removed, and the vessels secured by clean ligatures, any pedicle could be returned to the abdominal cavity with impunity. He was aware that this method of treating a pedicle had been tried by others with some success, but in the hands of Wells it had been a dismal failure. For what reason? Simply because the ligatures were not clean. The method was correct, but the technique was at fault.

In spite of what Tait said against Listerism, he had the good sense to know that his ligatures must be clean, and as he relied on boiling water to clean his instruments, so now he resorted to boiling water to make his silk clean; and because he did this, he scoffed at Lister and his antiseptics, and would not see that, in using boiling water, he was using the most powerful germ-killer that we know of.

As we have stated above, the recognition by Tait of the importance of a clean ligature, and the return of the ligature and pedicle to the abdomen, we hold was the beginning of modern abdominal surgery, for we shall show that it was Tait who extended the operative field from the pelvis right up to the diaphragm. But as it is the aim of this work to bring out the various antecedent stages of any procedure that Tait adopted, and next to show what influence he had on any procedure that he did adopt, we now propose to trace briefly the history of the ligature, and to show the various phases that the ligature passed through once it had been applied to the ovarian pedicle.

The history of the ligature is so intimately connected with the history of surgery, that we may say that its use or neglect may be taken as a guide to mark the progress or regress in the art of surgery.

Its first use, as a hæmostatic agent, was not the result of logical deduction; it was but the instinct that guided the primitive mind to stop the flow of blood, which must have appealed to the individual as having some intimate connection with life. The circulation of the blood did not enter into the empiricism of the primitive surgeon, and so the first words about the application of the ligature, for the purpose of preventing hæmorrhage, are given by Susruta, in the Ayur-veda (B.C. 1000), when he tells us that the umbilical cord was tied in newly born babes with a string eight inches from the navel before the cord was severed.

Archigenes (B.C. 100) used the ligature after amputation. Celsus (B.C. 30) in dealing with hæmorrhage directs us to seize upon the vessels that pour forth the blood and, having tied them up in two places, to cut them asunder, so that they may contract and still have their mouths shut up. If circumstances permit, then they are to be burned with a red-hot iron. Celsus used ordinary linen thread and he credits an obscure physician of the Alexandrian school with the discovery of the ligature.

Galen (A.D. 130), though not a practical surgeon, frequently mentions the ligature and gives directions about applying it to the proximal end of the bleeding vessel. For ligature material he advises silk and fine cat-gut.

The name of Antyllus (A.D. 300) is known to all with regard to his method of procedure in treating cases of aneurism by the ligature.

Oribasius does not mention the ligature in dealing with hæmorrhage; but Ætius, following Galen, recommends the ligature. Actuarius prefers astringents, but Palladius advocates the ligature.

Paulus Æginetus (A.D. 625) treats at length about the use of the ligature.

Albucasis mentions the cautery and the ligature, while Averrhoes stops bleeding by styptics, the cautery and the ligature; while Avicenna treats of all the modes of stopping hæmorrhage and recommends cold water, exposure to cold, escharotics, the actual cautery, and the ligature.

Rhases (A.D. 850) mentions the cautery, the application of snow, and the ligature; Hally Abbas does likewise.

Adams [1] says:

> The early modern writers in surgery make mention of all the ancient methods of stopping hæmorrhage. Guy of Cauliac recommends the ligature upon the authority of Galen and Avicenna. It is also recommended by Brunus, Theodoricus, Rolandus, and Lanfrancus. It appears, therefore, that the use of the ligature for stopping hæmorrhage was well understood by the ancients, and had never been lost sight of, even in the darkest ages.

We need but mention the names of some other surgeons who wrote about the use of the ligature: Vigo (1460), Ferri, Bolognini (1508) who used silk ligatures, and Houllier (1493). In Germany, Senn says that the first mention of the ligature is by Hans Von Pfolsprundt.[2]

To Ambrose Paré (1517) surgery owes a great debt of gratitude, not because he discovered the ligature, but because he was the first real champion of the ligature; and it was through his influence and untiring zeal that the ligature gradually found its way into popular favour and displaced the barbarous treatment by styptics and cautery.

In England the ligature was introduced by Wiseman (1566–1625), and was eagerly adopted after the circulation of the blood, by Harvey in 1619, had been made known.

Desault accidentally made the observation that, in the ordinary method of ligation with the round ligature, the two inner tunics of an artery are ruptured, and the fact was verified by Jones, who in 1806 made a series of careful experiments to determine the point. His work is classical, and it exerted a potent influence on establishing the claims of the ligature, not only in England, but wherever surgery was practised; this being the case, let us now trace the application of the ligature to the pedicle of ovarian tumours.

In his first operation of ovariotomy, in December, 1809, McDowell, of Kentucky, put a ligature around the pedicle, and the ends of the ligature were brought out through the lower end of the abdominal incision.

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[1] "Paulus Ægineta," vol. ii. p. 132.

[2] "Reference Handbook of the Medical Sciences," Buck, vol. iv. p. 490.

In 1821 Dr. Nathan Smith, of Connecticut, unaware that ovariotomy had ever been performed, tied the arteries in the pedicle, two in number, with leather ligatures, made from a kid glove, and also two arteries in the omentum, which he found adherent. All the ligatures were cut close, the parts returned and the incision closed throughout. Nathan Smith, therefore, was the first to adopt the complete intra-peritoneal ligature in connection with ovariotomy. His patient recovered.

Lizars of Edinburgh, in 1825, after performing ovariotomy, brought the ends of his ligature out through the wound.

In 1827 David Rogers, of New York, ligatured separately several large vessels in the pedicle of an ovarian cyst, and returned the stump of the pedicle with ligatures cut short; the operation was successful.

In 1835 Bellinger, of South Carolina, tied two arteries in the pedicle, in the same way as Nathan Smith, with animal ligatures and cut them close, with satisfactory results.

Jeaffreson, of Framlingham, removed an ovarian cyst, the ligatures were cut short, and the pedicle returned; the case was perfectly successful. This was in 1836.

Phillips performed an ovariotomy in 1840 and returned the pedicle with the ligature cut short; the patient died on the fourth day from the escape of blood from the pedicle.[1] The same fate befell a patient operated on by Stilling of Cassel, in 1837, after using the long ligature.

Siebold of Darmstadt followed the good practice of Nathan Smith with success; while Handyside of Edinburgh, and March, Greene, and Peaslee of America, carried the ends of the long ligatures out through the pouch of Douglas, instead of through the lower ends of the abdominal wound.

Clay (1842) of Manchester, who, according to Tait, first showed that ovariotomy could be made an operation more justifiable by its results than any of the major operations of surgery, always treated the pedicle by the long ligature.

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[1] Doran suggests that the patient did not die from hæmorrhage but from exhaustion due to choleraic diarrhœa. "St. Bartholomew's Hospital Reports," 1877, p. 198.

The pedicle was treated by the clamp method for the first time by Hutchinson on September 27, 1858.

Spencer Wells (1858) then came on the scene and used the clamp in hundreds of cases with a mortality that seldom fell below 25 per cent.

Baker Brown's contribution to the progress of ovariotomy was always held in high regard by Tait, who maintained [1] that Brown was the surgeon who established the intra-peritoneal method of treating the pedicle. Brown was not successful in his first attempts at ovariotomy, but when at last he determined (in 1864) to treat the pedicle by the cautery, and to return it to the peritoneal cavity, he got the most brilliant results, and his published results show a mortality of less than one in ten, and in his last forty operations he lost but four patients.[2]

In 1862 Thomas Keith began his operations in Edinburgh at the period when Tait was a student at the University. He very soon found that the results obtained by the clamp were extremely bad, so "he reintroduced and by his brilliant work has completely re-established the intra-peritoneal method of Baker Brown. With the clamp, his mortality was 19·2 per cent., which is close on that of Mr. Wells' 20·73; with the cautery, on the other hand, out of 156 cases, Dr. Keith has had a total mortality of only 3·85 per cent." (Tait.)

When Tait claims for Baker Brown the credit of establishing the intra-peritoneal method of treating the pedicle, he does not do justice to Tyler Smith, who, in 1861, when performing his eighth ovariotomy, transfixed the pedicle, cut the ligature short, and dropped the stump back into the peritoneal cavity.

This method was frequently used by Tyler Smith, and later on by Bryant. Knowsley Thornton [3] says, speaking of Tyler Smith: "To him undoubtedly belongs the credit of popularising this method with the profession, though he

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[1] *Med. Times and Gaz.*, March 26, 1881.
[2] Letter from Keith to the *Brit. Med. Jour.*, July 11, 1880.
[3] *Med. Times and Gaz.*, May 22, 1880.

cannot deprive our American brethren of the honour of first introducing it."

In 1868, Spiegelberg and Waldeyer published, in Virchow's "Archives," a very important series of experiments to show that foreign bodies, such as ligatures of silk, might be applied to the horns of the uterus of a bitch, and after the horns had been excised the ligatures might be left in the peritoneal cavity with impunity. They also showed that portion of the uterus could be removed by means of the cautery, and that the stump did no harm when left behind in the peritoneal cavity.

These researches had a very considerable influence, not only on continental surgeons, but upon English surgeons, and tended to strengthen the position of those who, like Doran and Thornton, became firm advocates for the intra-peritoneal treatment of the pedicle.

Tait has left us a passage in one of his works, in which he tells of his regrets at having used the clamp on the ovarian pedicle. He says:

> In 1858, Mr. Spencer Wells began his work, and in his third operation, he adopted the clamp as a method of dealing with the pedicle, and this he retained in all suitable cases until about 1878. During this time he performed 627 operations with the clamp, having a mortality of 20·73 per cent. During the same period he operated 157 times with the ligature, with a mortality of 38·2 per cent., and on this point it is to be noted that he retained the intra-peritoneal method only for three cases to which he could not apply the clamp. I mention this here merely to indicate my opinion that the introduction of the clamp was a decidedly retrograde step in the history of ovariotomy. When I began my practice in 1867, I employed the écraseur, a variety of the intra-peritoneal method, and my results over a limited experience were extremely good. Like others, however, I was so impressed with the overwhelming experience of Mr. Wells, that I resorted to the clamp, and my results with it were so bad, that its employment will ever be to me a matter for bitter and lasting regret.

In another place he says:

> Mr. Spencer Wells exercised an impregnable influence on the conduct of ovariotomy, owing to the accident of circumstance, and with his mortality of twenty-five per cent., any

real advance of abdominal surgery was wholly impossible. No one could venture to submit a woman to such a painful risk, unless her life was clearly menaced, and therefore ovariotomy was always delayed as long as possible; palliative tappings, and other blunders were perpetrated; but, worse than all, the diseases in the pelvis and abdomen, which were amenable to surgical treatment, but which did not immediately and evidently destroy life, were left alone, and the patients went unrelieved. Dr. Thomas Keith ended this dark period by showing us how to operate on the abdomen without fear and with little risk, and these recent advances in pelvic and abdominal surgery should in great measure be placed to his credit, though I do not know that he has engaged in them in his own personal practice to any large extent.

For my own part, so fearless am I now of abdominal surgery, so splendid have been my results in fields of practice which, until three years ago, seemed hopelessly enclosed, that I venture to lay down a surgical law, that in every case of disease in the abdomen or pelvis, in which the health is destroyed or life threatened, and in which the condition is not evidently due to malignant diseases, an exploration of the cavity should be made.

These lines were written in the early part of 1881, and will give us a good idea of the extraordinary change that had come over Tait's work since 1878, when he had published his first series of fifty ovariotomies with nineteen deaths; and these lines mark the point from which general abdominal surgery started. Exploration when in doubt was the only way to decide the condition, and exploration in time gave experience, and so exploration became less necessary as the years passed by; but never so long as abdominal surgery exists shall we be independent of explorations, first advocated and largely practised by Lawson Tait.

Let us repeat, the great light that had come to Tait at this period was the assurance of success by the intra-peritoneal treatment of the pedicle with the ligature, and in addition to this, as he stated in one of his papers, the necessity of "scrupulous attention to cleanliness of every kind and in all directions."

In the last year of his life, Tait [1] even went so far as to

[1] "The Evolution of the Surgical Treatment of the Broad Ligament Pedicle," the *Lancet*, May 16, 1896.

claim that Bancroft and himself had reintroduced and reinstalled the short ligature of the pedicle in 1878. One cannot admit Tait's claim, for the following passages from Doran, Fergusson, and Bantock, will show that the short ligature was being used on all sides in 1877. Doran says, in a paper dealing with complete intra-peritoneal ligature of the pedicle in ovariotomy [1]: "An important section of contemporary medical literature furnishes us with a strong proof that complete intra-peritoneal ligature of the ovarian pedicle is firmly established."

Sir William Fergusson in his "System of Practical Surgery" (5th edition, 1870), says:

> In recent times the plan of cutting off both ends of a ligature has been most satisfactorily applied to ovariotomy, particularly by Dr. Tyler Smith. The neck of the tumour has been surrounded by a stout thread, and after a light noose has been drawn the ends of the ligature have been cut close to the knot and the pedicle has been allowed to glide into the peritoneal cavity. I have tried this process in my own practice, and it has answered admirably, nor am I aware of any instance where evil has been attributed to it.

Again, Bancroft, in a discussion on Intra-peritoneal Myomectomy at the British Gynæcological Society in 1893, said, in reference to the treatment of the ovarian pedicle:

> In my fourth, fifth, and sixth cases I used the clamp, and it was a logical deduction that if the ligature was good enough for the bad cases, it was, *a fortiori*, also good enough for the favourable cases.
>
> It was under these circumstances, then, that I publicly announced at the Samaritan Free Hospital that I would not again use the clamp until I had given the ligature a fair trial and it had failed me. Since that date, viz. November, 1875, I have never used anything but the ligature, yet the clamp continued in use till the following July, when Mr. Thornton also abandoned it, and the use of the ligature became invariable with us. But, waiting until I had accumulated overwhelming evidence, I was anticipated in the matter of publication, and hence I have not obtained the credit that I consider my due.

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[1] "St. Bartholomew's Hospital Reports," 1877, p. 1991.

Tait in his treatment of the pedicle at this period was using silk, but he was desirous of seeing Keith's work, so he went to Edinburgh with that end in view. He says :

> As I watched Keith's operations in 1880–1881 I became convinced that the cautery was the ideal method of pedicle treatment, but for the tremendous time it occupied, and the large amount of physical exertion involved. . . . I do not think that either Baker Brown or Keith understood how they arrived at their certain and magnificent results. I am sure the bystanders did not. Every one whom I have asked questions has answered : " Oh yes, sear the stump : barbarous practice, going back to the days before Ambrose Paré."

But Keith did not sear the stump. It is true that he burnt a piece of it off after securing it with Baker Brown's clamp, and if searing had been the means of his success he would have stopped there. But he went on for about twenty minutes or half an hour, rubbing the clamp with his cautery and clearing it with a towel or sponge until the onlookers got weary of the proceeding, and thought Keith was finical. What he had done was really this—and I found it out after much experimenting—that he had seized a transverse strip of the pedicle between the iron blades of his clamp, screwed the blades up tight, and then heated his blades up to cooking point—that is, practically between 180° and 190° F.—and carefully maintained that temperature till the enclosed strip of pedicle was cooked dry into a strip like parchment. Tait says that, having watched the process, " Keith and I agreed to stick each to his method, and after a long series of cases to compare results."

The facts set forth above show us that it was the use of the short ligature, rendered clean by antiseptics, or by boiling, that determined the fate of abdominal surgery ; that Tait came on the scene when abdominal surgery meant merely the removal of ovarian tumours ; that after using the clamp with disastrous results he adopted the use of the short ligature ; that his great achievement was to give the first impulse to the further development of abdominal surgery, for he left the confined field of ovarian cyst surgery

to wander over the whole abdomen, attacking one diseased organ after another, and thus established the surgery of the gall-bladder, the Fallopian tubes, and the broad ligaments. For this reason we claim that Tait was the founder of modern abdominal surgery.

XVII

Pelvic Suppuration and Drainage

1880

At the May meeting of the Royal Medical and Chirurgical Society, Tait read a communication on the "Treatment of Pelvic Suppuration by Abdominal Section and Drainage." [1] The paper was illustrated by the history of six cases on which Tait had successfully operated.

The cases recorded were those in which the suppuration had occurred in pelvic hæmatoceles. The reason that led him to adopt abdominal section in the treatment of this form of pelvic suppuration he explains thus:

> Like others who follow the surgical specialty in which my practice lies, I have had a wide field for the observation of the various conditions classed under the head of pelvic abscess, and like others, I have until recently confined my treatment of it to openings made from the vagina or in the neighbourhood of Poupart's ligament. Experience, however, has driven one to the conclusion of Dr. Emmett, that "I cannot regard the introduction of the trocar into the inflamed tissues of the pelvis as a procedure free from danger under all circumstances." It is perfectly true that in very many cases where an abscess undoubtedly exists in the cellular tissue of the female pelvis the fluid can be reached and removed by the needle of the aspirator. But, according to my experience, the relief obtained in this way is, in a large number of cases, neither complete nor permanent, and, in nearly all, the convalescence has occupied a time not at all commensurate with the extent of the lesion. This is quite as true of abscesses which have been allowed to open themselves, or have been assisted to open in the groin. They often continue as fistulous openings for years.

Tait then gives West's description of the course of pelvic

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[1] This paper was reproduced in "Diseases of the Ovaries," 4th edit., 1883, p. 344.

abscesses, and he remarks that West's words cannot be improved upon, and after this he says :

> In my own practice such disappointing cases have occurred with but too great frequency, and though I have had some successes by the employment of such means as the elastic ligature (the *Lancet*, June 7, 1874) and counter-opening in the vagina (the *Lancet*, April 3, 1875), yet the progress towards recovery has been so protracted as to contrast favourably only with those cases in which there was no recovery at all.
>
> I have been, therefore, continually on the outlook for some means of dealing with such cases which would bring them as satisfactorily within our means of treatment as are collections of matter in most other parts of the body. This has been furnished by the wide, free, and successful application of abdominal section for the treatment of pelvic and abdominal tumours, and I have now to lay before the Society six cases, which include the whole of my experience in this novel proceeding, and in which success has been obtained far surpassing any that I have yet seen or heard of. In this comparison, I am, of course, excluding those cases whenever pointing of the abscess in the vagina is evident at an early stage of the case ; but even in these, the recovery has always been, in my experience ; more protracted than in these six now to be narrated.

The first case that Tait operated on will give us a good idea of his new method. The patient was twenty-two years of age, she had been married nine months, and six weeks before Tait saw her menstruation had stopped suddenly during its course, and this was associated with the onset of violent pelvic pain—which, Tait remarks, is one of the leading features of extra-peritoneal hæmatocele. A few days after the periods ceased she shivered and became ill and feverish. When he examined her he found that she was emaciated, with a high night temperature, intense pain and tenderness over the lower abdomen, and, when examined, a large fluctuating tumour, adherent to, and behind the uterus, and going on either side of it, was found to occupy the pelvis and rise about halfway up to the umbilicus. The roof of the pelvis was fixed and hard, and no fluctuation could be felt therein. Tait came to the conclusion that he had to deal with a suppurating hæmatocele, and he determined to open it from above. He says :

> I found a large cavity, containing about two pints of fetid pus with decomposing blood-clots. This I carefully cleansed out, and after having united the edges of the opening into the cyst carefully to the abdominal wound, I fixed in one of Koeberlé's glass drainage tubes, five inches long. Seven days after the operation, I placed a three-inch glass drainage tube, and in another week this was replaced by a soft rubber tube. The patient got up on the twentieth day after the operation, and in ten days more went home perfectly well and with the abscess healed. She remains now in perfect health (March, 1880).

This operation was performed in February, 1879, and during the next twelve months Tait was able to operate successfully on five others, and in 1883, when he republished his paper, he added a note to the effect that, "since the original publication of the paper, I have operated on fifteen cases, and the recovery in all the twenty has been complete and permanent."

When we come to consider the unsatisfactory condition of abdominal surgery at this period, we must give Tait full credit for this bold piece of original work. To deliberately open the abdominal cavity, and attack a sac filled with pus, was a procedure that only a man of courage and originality, like Tait, would have attempted.

A discussion followed the reading of this paper, and Erichsen, the President, said that the paper was a valuable contribution to abdominal surgery, opening up a new field in a department which had made more advance than any other in surgery.

Knowsley Thornton said that there could be little doubt that the measures described were a great advance in the treatment of a very intractable class of cases. He, himself, had twice resorted to the same plan.

Spencer Wells hoped Mr. Tait would say what cases should be dealt with by abdominal section in preference to puncture by a large trocar through the vagina, and treatment by injections of carbolised or iodised water. Seeing how readily such cases could be treated through the vagina, and free drainage thus obtained, he would hesitate before opening the peritoneal cavity, involving further risk to the patient.

Tait, in his reply, first answered some questions put by the President, Erichsen, who wanted to know, "Whether antiseptic precautions were taken; the size of the glass tubes employed; and whether any precautions were taken to exclude air?"

Tait said that he could not give any precise information as to the antiseptic precautions, for he had ceased to regard the influence of antiseptic treatment as of any account. He believed the first four cases were treated antiseptically during, but not after, the operation. In the other two, no antiseptics were used at any time; and in the sixth case,

> all septic symptoms ceased as soon as the abscess was evacuated, and air had free access into its cavity. The glass tubes were those introduced by M. Koeberlé; they measured from six to eight inches in length. In no case were there any peritoneal adhesions. He invariably made his incision in the middle line, and in five out of the six cases the peritoneal cavity was opened. In no case was any difficulty met with in securing the wall of the abscess to the parietes, but of course great care was exercised. Mr. Wells had a very proper and conservative regard for the peritoneum, but he, Tait, had ceased to fear any operative measures dealing with the peritoneum. The advantage he claimed for the operation in question was the rapidity with which the abscesses, so dealt with, healed. At present he might limit the operation to cases when the abscess could be better got at from above than through the vagina; but he was not sure that they should stop there, seeing how long and tedious was recovery after the necessarily limited incision made through the vagina.[1]

Rotation of Ovarian Tumours

1880

At the meeting of the Obstetrical Society of London, March 3, 1883, Tait read a paper "On Axial Rotation of Ovarian Tumours, leading to their Strangulation and Gangrene." He recounted the particulars of three cases that were successfully treated by the performance of immediate ovariotomy. In all three cases the tumour was on the right side, and the pedicle was twisted *from within outwards and to the right side.* From this circumstance,

[1] The *Lancet*, May 22, 1880, p. 805.

Tait deduced a theory as to the causation of the rotation—namely, that it is due to the alternate filling and emptying of the rectum : the fæces descending the rectum would act as a wedge, and the point of application of the force would be the most favourable possible in the case of a right-side tumour.

Torsion of the ovarian pedicle was a subject that Tait always took an interest in, and in discussing the subject with us in 1892, he still held the same views with regard to the causation of the rotation, *i.e.* that it was due to the alternate filling and emptying of the rectum.

Torsion may occur at any age ; Thornton observed it in a thirteen-year-old girl, and Potter in a woman aged eighty-three years (Palmer Findley).

Tait [1] has given us a sketch of what had already been written about rotation of the pedicle. He shows that the first notice of the incident is made by Rokitansky, who describes it in his " Handbuch der Pathologischen Anatomie," in 1841.

Thomas stated that it was described by Hardy in 1845,[2] Ribbentrop in 1846, and Van Buren in 1850.[3]

Patruban, in 1855, published a case when the torsion produced rapidly fatal intra-cystic hæmorrhage.[4]

Crome, of Brooklyn,[5] recorded a case where the strangulation occurred twenty-four hours before labour.

The subject was most fully analysed by Rokitansky in the third edition of his text-book, and in the *Wiener Allgemeine Med. Zeitung*, in 1860.

Spencer Wells dealt with the subject in his " Diseases of the Ovaries " in 1872. Tait contributed his paper in 1880. Alban Doran devoted a chapter to the subject in his work on " Tumours of the Ovary," etc., in 1884 ; while Knowsley Thornton, in 1888, contributed the most elaborate paper on the subject.[6]

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[1] " Diseases of the Ovaries," 1883, p. 295.
[2] The *Lancet*, April, 1845.
[3] *New York Journal*, March, 1850.
[4] " Oesterreiches Zeitschrift für practische Heilkunde," 1855.
[5] *American Medical Monthly*, 1861.
[6] " Rotation of Ovarian Tumours, its Etiology, Pathology, Diagnosis and Treatment," *Amer. Jour. Med. Scs.*, October, 1888.

With regard to the mechanism by which this singular rotation is produced, Tait considers that we may at once dismiss any explanation which attributes it to the condition of the tumours themselves, for we find it occurring in tumours of all kinds, large and small, smooth and globular, multicystic and irregular, parovarian, ovarian, dermoid, and solid fibrous tumours. The only intrinsic conditions of the tumours being that they should be free to move, and have pedicles capable of being twisted.

Tait refers to the conclusion arrived at by Klob, who, after making some experiments, thought that it was the alternate filling and evacuation of the bladder which rotates the tumours; Tait thought that on *a priori* grounds that there was something in the idea. His objection to the theory was that the bladder alone would not be likely to do it, being situated in the centre line, its influence would be, in all probability, neutral. He was inclined to think that the bladder might help, but that the alternate filling and emptying of the rectum was the real cause of the rotation.

> If (he says) it were the rectum, then the force acting on the left side of the point of rest, the vertebral column, would inevitably push the tumour in the direction of which, in at least nine out of ten of my cases, the movement took place; and it would certainly act more readily on right-side tumours than those on the left side, for the former are anchored so that the pushing force of the rectum will be in the requisite oblique direction, in the place of a screw, and very nearly at right angles to the axis of movement.

Alban Doran supported Tait's theory, and wrote:

> I cannot see that the similar theories propounded by Mr. Tait and myself can be in any way unreasonable, based as they are on actual observations; still, I believe that, as a rule, the twisting of a pedicle is to be explained by the simpler doctrine that the tumour, pressed upon by the viscera and even the costal cartilages above, and by the pelvic structure below, but comparatively free laterally and anteriorly, rotates on its own axis every time that the patient, after walking, or lying on her back, turns round and rests on her side.

Herman [1] agrees with Tait's theory, and says, "I know of no collection of cases sufficient to disprove Tait's reasoning."

The chief objector to Tait's theory was Olshausen, who pointed out that Tait had largely relied on the fact that four-fifths of Rokitansky's cases, and all Tait's cases, had been tumours of the right side; but Olshausen showed "that fifteen of my last nineteen cases of torsion occurred in tumours of the left ovary." [2]

Howard Kelly has recently written: [3]

> I find two causes for the rotation of cysts. In the first place, large multilocular cysts exhibit a notable tendency to the formation of one large cyst cavity, with a number of subsidiary ones, and a movable tumour will invariably turn until the convex surface of the large cyst comes to lie in relation to the concavity of the distended anterior abdominal wall; this produces a partial rotation, which does not tend to increase. I think the cases in which the rotation amounts to or exceeds one turn, found in the spherical tumours of the size of a man's head, are for the most part due to the alternate relaxations and contractions of the anterior abdominal walls acting most decidedly on the part of the tumour which is nearest the median line. The tumour, which, as a rule, does not lie exactly in the median line, is then nipped from time to time between the linea alba and projecting vertebral column.

Investigations carried on after the time of Tait show us that both pregnancy and labour have a very marked interest in regard to torsion of the pedicle, for torsion occurs almost thrice as often in association with these events as at other times. Torsion often occurs during the third stage of labour, when the diminished size of the uterus allows greater movement of the abdominal contents.

Apart from pregnancy, torsion of the pedicle occurs in about 8 per cent. of ovarian tumours, during pregnancy in 12 per cent., and in association with labour and the puerperium in 22·7 per cent. It is commoner with abdominal than with pelvic tumours. The greater frequency of torsion of the pedicle during pregnancy is probably due to the fact that the enlarging uterus exerts pressure on one part of the

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[1] "Diseases of Women," 1898, p. 754.
[2] Olshausen, "Diseases of the Ovaries," 1887, p. 92, Wood, New York.
[3] "Operative Gynæcology," 2nd edit., 1899, vol. ii. p. 278.

tumour only and so causes axial rotation. During labour it must be attributed to contraction of the uterus, and after delivery to the rapid diminution in the size of the uterus and to the movement thus imparted to the tumour as it returns again to the cavity of the pelvis.[1]

Before leaving this subject, we might record the fact that, in regard to the operation treatment of this condition, Wiltshire, in 1868, operated on a woman suffering from symptoms of the utmost gravity, due to peritonitis, arising from gangrene of an ovarian tumour; it having rotated on its pedicle four days before the operation.[2] Tait gave great praise to Wiltshire for his courage in performing the operation under such urgent conditions. The patient recovered.

Oophorectomy or Removal of the Uterine Appendages

1880

During 1880 Tait frequently removed the appendages and the ovaries for various diseases.

In the *British Medical Journal*, July 10, 1880, he gives a list of all the cases on which he had operated.

He says:

> The discussion which I have just read in the section of Obstetrics and Diseases of Women, of the American Medical Association, on June 1, shows conclusively that the operation is accepted amongst our transatlantic brethren, as not only one that is to be justified, but one that is welcomed as an immense addition to our power of relieving suffering women. In this country it has not yet achieved such a position, and therefore every one engaged in its practice is bound fully to disclose his experiences, and not to publish only successful operations.
>
> To establish this operation, unlike many others, two steps are required. The first is to show that its immediate results are sufficiently free from danger to justify its performance merely as a matter of risk. The second is to show, by its more remote results, that it does really confer upon the

[1] Williamson and Barris in Eden and Lockyer's "New System of Gynæcology," vol. ii. 1917.

[2] *Trans. Path. Soc.*, 1868.

> sufferers the relief which theoretically it promises. I therefore propose to give here a skeleton history of all the cases I have performed since the revival of the operation. Full particulars will be given for each case in future communications, which I hope to make to the *Journal*. In the meantime, the following experiences—larger, I think, than that of any other operator but Professor Hegar—will, I think, be enough to show that the operation is far more free from risk than many others long since accepted, and which have had, in my opinion, far less satisfactory results in the way of permanent relief.

Tait then gives a table of twenty-six complete operations, and two incomplete operations.

This table shows that the operations were performed for dysmenorrhœa, menstrual epilepsy and mania, menorrhagia hæmorrhage from myoma, menorrhagia due to hypertrophied ovaries, and abscess of the ovary.

> In twenty-five completed operations there was only one death, and I have to regret ever having attempted the operation. The patient in that case was almost moribund from hæmorrhage when I saw her, and I certainly would not undertake such a case again.
>
> The death among the incomplete operations warned me that unless the ovaries can be fully removed, they had better not be touched.

A few weeks after the publication of this short paper, Tait was invited to address the members of the West Somerset Branch of the British Medical Association, at their Annual Meeting at Wellington, and he did so, reading a paper on "Oophorectomy in Cases of Dysmenorrhœa." [1]

Tait, in this paper, claimed for the relief of dysmenorrhœa by oophorectomy the following advantages: comparative safety of the operation, as no fatal results had followed the operation in his hands; immediate, or almost immediate, benefit to the patient; pain and hæmorrhage being in every case alleviated, and in the majority of the cases entirely removed; great improvement in the bodily condition and general health of the patient; restoration, in the case of women depending for their living on labour, to a position in which they are enabled to remain in domestic service, or

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[1] *Brit. Med. Jour.*, August 7, 1880.

discharge other labours, without the interruption, as heretofore, of several days in each month.

Tait took the opportunity to rebut the various objections which had been brought, or might possibly be preferred, against the operation, and he dealt with the following points : (1) unsexing the patient ; (2) destroying the sexual instinct and desire in women ; (3) alteration of voice and appearance ; (4) criminal abuse of the operation.

A few weeks later Tait read a communication to the Psychological Section of the British Medical Association, at the Annual Meeting held at Cambridge, on a case of *Menstrual Epileptic Mania*, treated by oophorectomy with complete success. Crichton Brown was President of the section, and was favourable to the surgical treatment of such cases.

At this meeting of the Association, Spencer Wells read a paper on the " Removal of Uterine Tumours," and Tait discussed the paper, and strongly urged that, in the treatment of uterine tumours, the question of the removal of the ovaries should first be considered, because, by their removal, the hæmorrhage was completely arrested. He announced that in future he would seldom employ enucleation or the removal of the tumours, unless the tumour was of great size and was pressing injuriously upon some organ.

At this time Tait had had some experience in the removal of the ovaries in order to check excessive hæmorrhage, and had operated in some fourteen cases with only one death.

During this year he operated on three cases of hydrosalpinx, and three cases of pyosalpinx, with recovery in each case, and he performed hysterectomy for myoma, the pedicle being treated by a circular wire clamp which he had invented. This operation was performed in September, 1880.

Up to this time, Tait had had but little success when he had tried to remove myoma from the uterus by abdominal section ; five out of his nine cases had died.

He says :

> In Mr. Spencer Wells' recently published book is given a list of thirty-nine cases of removal of uterine tumours with

twenty deaths. This terrible mortality would be more than enough to condemn this operation, and up to a short time ago, in my own practice, the results were nearly as bad, and I did condemn it very strongly. I found, however, from my failures, that it was, as in ovariotomy, the method of dealing with the pedicle which was at fault, and now that I have altered and amended this, I have obtained results which seem to promise that I shall do almost as well in the removal of uterine tumours as I have done in ovariotomy.

Of the twenty deaths in Mr. Wells' list, fifteen occurred with the use of the ligature for the treatment of the pedicle, and only one with the clamp. My experience is quite in harmony with this. I found when I used the ligature, however tightly it was tied, and however carefully I stitched it up like a stump, as advised by Mr. Wells, the stump shrank in a few hours from the escape of serum, the ligatures loosened, and the patient died of hæmorrhage. In only two cases have I been successful with the ligature, and in both I added the cautery, and did not sew up the stump.

I found that Koeberlé's plan was open to objections, because the small size of the wire cut the pedicle, and gave rise to secondary hæmorrhage (in two cases), and that (in one case) the wire broke some hours after the operation and allowed fatal hæmorrhage to occur.

Tait then set to to devise a clamp of his own, and this proved completely successful in a series of cases; his first operation being performed on September 8, 1880.

A letter to the *British Medical Journal* [1] shows that Tait was still an active member of the Health Committee of the Corporation of Birmingham, and that he had no hesitation in disclosing the operation results obtained in the Birmingham hospitals in regard to ovariotomy. He says:

Mr. Solomon's allusion to the Birmingham Hospital for Women is a most unfortunate choice. He says our mortality exceeded anything that has ever been known in the annals of the operative surgery of Birmingham. And yet, all the statistics that have been published are within easy access to Mr. Solomon. He either must, or ought to, know that during the time he speaks of, there were forty ovariotomies performed there with fifteen deaths; a mortality of 37·6 per cent., bad enough in all conscience. But up to the same time, the combined experience of the same operation, performed in the two large hospitals in Birmingham, the Queen's and the

[1] September 25, 1880.

General, shows thirty-one operations with twenty-three deaths, or a mortality of 74 per cent. I think the officers of this institution will hardly be grateful to Mr. Solomon for thus most wantonly obliging me to use these figures. They have been long in my possession, and I have never used them before, nor did I wish to do so now.

XVIII

The Influence of Tait's Surgery on the Pathology of the Lower Abdomen

1880

We have seen in the preceding chapters that Tait had now operated on cases with hæmatosalpinx, hydrosalpinx, and pyosalpinx, and had also attacked collections of pus in the broad ligaments by abdominal section. Let us try and show the effect these procedures had on the pathological views held in regard to pelvic inflammation and suppuration.

Howard Kelly, in 1907, said :

> that the treatment of pelvic abscess is fraught with the deepest interest, for it exhibits in miniature all the phases of the history of the growth of gynæcology at large. It had its genesis in the timid conservatism of thirty years ago, and then stepping out of swaddling clothes it advances through the phases of the most radical methods in abdominal surgery, to terminate in a sound conservatism allied to a bold radicalism, applied in properly selected cases. The older method of treating pelvic abscess was based on false pathology, giving rise to the conception that there was a suppurative process going on in the cellular tissues of the broad ligament.[1]

In order to understand how Tait's operations completely revolutionised not only the pathology of the lower abdomen, but also the whole method of treatment, we must go back many years and glance at the erroneous views held since the beginning of the eighteenth century on inflammation and suppuration in the pelvis.

Herman [2] considers that Mauriceau, in 1740, was the first to describe pelvic cellulitis. Before his time these cases

[1] "Gynæcology and Abdominal Surgery," Kelly and Noble, vol. i. p. 650.

[2] *Brit. Med. Jour.*, May 26, 1900, p. 1273. Mauriceau was born in 1637 and died 1709. His "Traité des Maladies des femmes grosses" was published in 1675, and a ninth edition as late as 1740. Pozzi says, "Mauriceau attribudit toutes les tumeurs post-puérperales à la rétention des lochies."

were either called abscess of the womb, or the nature of the diseases was not recognised. Mauriceau was the first to describe the abscess as not situated *in* the womb, but at *the side* of the womb. McClintock [1] has, however, drawn attention to the fact that Richard Wiseman, Sergeant-Chirurgeon to King Charles the Second, has described cases of puerperal pelvic cellulitis ending in abscess, attributing them primarily to " distempers of the uterus in childhood."

In 1759, Puzos,[2] in his " Traité des Accouchements," devoted much space to a " Mémoire sur les Dépôts laiteux," and he expressed the view that pelvic exudations were the result of a metastasis of milk. He locates these " milky deposits " in the iliac fossa under the skin and the fat, and between the muscles and peritoneum. The most important are lodged in the cellular tissue, the peritoneum, or the broad ligaments, or in the ovaries. For Puzos, the disease was pelvic only by an unexplained accident, and it was not uterine nor ovarian in its origin.[3]

Such opinions were for a long time entertained, and even William Hunter is said to have taught them, and Mathews Duncan has suggested that the opinion of Grisolle and Bennet, that sudden arrest of lactation may cause iliac abscess, is probably a remaining vestige of the extinct pathology of Puzos.[4]

Dupuyten [5] attacked the subject, but added nothing new ; he remarks that after labour engorgements are frequently observed in one or other iliac fossa ; it is neither pelvic nor uterine, nor ovarian.

Grisolle, in 1839, published papers on abscess of the iliac fossa. He says nothing of intra-pelvic abscess, and he takes great care to try and show that inflammation of the uterus,

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[1] " Clinical Memoirs of Diseases of Women," 1863, p. 1.

[2] Thomas says " Drs. West and McClintock date the appearance of Puzos' ' Traité d'Accouchement,' 1759. They are probably in error, as Bernutz and Nonat both date it 1743." Thomas is wrong because Puzos died in 1753, and his work did not appear until 1759, when it was published in Paris.

[3] Mathews Duncan, "Perimetritis," Edinburgh, 1869, p. 14.

[4] The same ideas were upheld on the Continent by Ritgen, A. E. Siebold, and Busch.

[5] " Clinique Chirurgicale," t. iii., p. 530.

and its appendages, had nothing to do with the collection of diseases which he grouped under iliac abscess.[1] A paper by Bourdon[2] shows that phlegmon of the broad ligament was still confused with abscess of the iliac fossa of totally different origin.

It is to Doherty,[3] of Galway, that we give the honour of the commencement of our modern views of these pathological conditions, and he was quickly followed by Churchill,[4] of Dublin: the former gave his views in a paper entitled "Chronic Inflammation of the Uterine Appendages occurring after Parturition," and the latter under that of "Inflammation and Abscess of the Uterine Appendages." Doherty pointed out some of the most important signs of pelvic inflammation, which are to be recognised by vaginal examination, "the hardness which is tender to the touch, and as firm and inelastic as a deal board"; also the fixation and displacement of the uterus, which is bound down to the affected side. Both these men clearly declared the occurrence of exudations close to the uterus and within the limits of the true pelvis. Both considered the subject simply as pelvic inflammation. Neither undertook to discuss the tissues involved in different forms of inflammation.[5]

The next important contribution is the work of Marchal (de Calvi), entitled, "Des Abcès phlegmoneux intrapelviens," published in 1844. This little volume far exceeds in importance all previous writing on the subject. First of all, Marchal gets rid of the erroneous connection of the disease by name with the iliac fossa. He calls the abscesses intra-pelvic, yet he does not simply describe them as intra-pelvic; he goes further and specifies the different tissues involved in the inflammation. Thus from autopsical evidence he declares the existence of, (1) abscess of the subperitoneal cellular tissue; (2) abscess of the subaponeurotic space; (3) ovarian abscess; (4) intra-peritoneal abscess. He also

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[1] *Archives Générales de Médecine*, iii. Serie, t. iv. p. 50, 1839.
[2] *Revue Médicale*, 1841, t. iii. p. 5.
[3] *Dublin Journal of Medical Science*, 1843, vol. xxii.
[4] *Dublin Journal of Medical Science*, 1843, vol. xxiv.
[5] Maury, "System of Gynæcology," Mann, Edinburgh, 1877, vol. i. p. 676.

recognises the difficulty of distinguishing, during life, an abscess which is the result of a cellulitis from those purulent deposits which arise from inflammation of the pelvic peritoneum.

As time goes on we find different opinions in regard to the tissues usually involved in pelvic inflammation. Auguste Nonat, in 1850, insisted that inflammatory swellings in the pelvis were all located in the cellular tissue, and that the peritoneum was not involved. To these swellings he applied the term " peri-uterine phlegmon."

Nonat,[1] Valleix, and their pupils, advanced the clinical knowledge of peri-uterine inflammation by describing the more sharply limited collections of pus, which are found posterior to, and upon the sides of the uterus; they considered them to be situated in the cellular tissue, which they thought existed not only between the folds of the broad ligament, but around the supra-vaginal portion of the cervix.

After this came the important observation of Bernutz. The work of Bernutz stands out as superior to everything done before the time when Tait began to operate. Bernutz was a most keen clinical observer. He was led to investigate the subject after studying Nonat's observations on peri-uterine phlegmon, and after examining certain patients who had peri-uterine swelling, and who presented all the symptoms of the so-called peri-uterine phlegmon situated in the cellular tissue. Bernutz discovered when the patients died that instead of finding an inflammation in the cellular tissue of the broad ligaments, he was able to demonstrate that the swellings that he had observed in his vaginal examinations were " formed by the pelvic viscera being matted together by peritoneal adhesions, and I conclude that inflammation of the pelvic serous membrane is always symptomatic, and that it is generally symptomatic of inflammation of the ovaries or Fallopian tubes." Bernutz

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[1] Nonat, *Gaz. des Hôp.*, 1850, p. 97. Martin, Thèse de Paris, 1851; Valleix, " De l'inflammation du tissu cellulaire peri-utérin," Union Méd., 1853, 5e sér., t. ix. p. 285; Gallard, " De l'inflammation du tissu cellulaire qui entoure la matrice," Thèse de Paris, 1855.

maintained that the cellular tissue was usually not to any degree invaded.[1]

A few years later, in 1868, Gaillard Thomas, in his well-known "Practical Treatise on the Diseases of Women," presented the subject in a very excellent way. He accepted the teachings of Bernutz, and acknowledged the independent existence of two distinct forms of pelvic peritonitis, and he said that while they frequently co-existed, they were entirely distinct from each other.

Emmet, the other well-known American gynæcologist, however, was a great believer in cellulitis, and said about this period: "I do not exaggerate when I assert that pelvic cellulitis is by far the most important disease with which woman is affected."

In 1869, Mathews Duncan published his monograph entitled "Perimetritis and Parametritis." In this he recognised two distinct forms of peri-uterine inflammation. He rejected the term *pelvic cellutitis*, but following Virchow, who proposed to use the affix *peri*, to imply inflammation of serous membrane, and *para*, to imply inflammation of cellular or connective tissue, he brought into use [2] the terms *perimetritis* and *parametritis*; the former to imply inflammation of the uterine peritoneum, and the latter to imply inflammation of the cellular tissue in connection with the uterus; and he proposes to use the terms *perimetric inflammation* and *perimetritis*, *parametric inflammation* and *parametritis*, to imply inflammations which directly owe their origin to disease of or injury of the uterus, tubes, or ovaries.

Duncan accepts the views put forward by Bernutz with regard to perimetritis, but does not agree with him about parametritis; and while Duncan acknowledges that disease of the tubes may cause both perimetritis and parametritis, yet he does not attribute to the tubes the important part

[1] Bernutz and Goupil, "Clinical Memoirs of the Diseases of Women," transcribed by Meadows. London, 1867, vol. ii. p. 5.

[2] Pozzi, quoting P. Delbet ("Des suppurations pelviennes chez la femme," Paris, 1891), says that Virchow, not Duncan, created the words *perimetritis* and *parametritis*. Duncan himself says in one place, "Virchow has made a contribution to the pathology of this subject. The affection we are now discussing he first named *parametritis*."

in the etiology which we know they hold now. Aran, on the other hand, in his description of peri-uterine inflammation leaves us in no doubt what, in his opinion, is the cause of it, for he says, "The causes of peri-uterine inflammation are those of inflammation of the tube and of the ovary, which is almost constantly its starting point." [1]

West about this period also held similar views, for he says: "The tendency of recent investigations, too, is to show that here, or rather in the ovary and Fallopian tubes of one or other side, is the almost invariable starting point of the mischief, be the parts involved in it what they may." [2]

Here, then, we have the outline of a subject about which more was written than about any other subject connected with gynæcology up till the year 1880, when Tait began to perform his operations on the Fallopian tubes. These operations gave the death blow to those who, like Emmett, had persisted in the belief that the trouble was so frequently located in the cellular tissues at the side of the uterus.

The difference between the old faith and the new has been well described by Taylor, at one time Tait's assistant, who, in a clinical lecture on Pyosalpinx at Birmingham, said:

> The specimen before us explains much that is difficult to reconcile between the old faith and the new on the subjects of parametritis and perimetritis. Briefly stated, the old faith in these diseases was as follows: Parametritis is an inflammation of the cellular tissue of the broad ligament; it is a common disease; it generally terminates in complete resolution; it frequently ends in abscess; it is rarely fatal. Perimetritis is inflammation of the peritoneum around the uterus and its appendages. Except for a form of very mild and limited character, which has no symptoms and may be ignored, perimetritis is a rather rare disease. It is usually accompanied by grave and "so-called" septic symptoms. It is not infrequently fatal.
>
> The new faith is as follows: Parametritis as the name of a common disease is altogether a misnomer; it is occasionally met with, but is almost a pathological curiosity. Perimetritis is salpingitis. Salpingitis is the essential disease, which has been wrongly or inadequately labelled as parametritis or perimetritis.[3]

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[1] Aran, "Leçons Cliniques sur les Maladies de l'Utérus," p. 718.
[2] West, "Lectures on the Diseases of Women," 3rd edit. p. 418.
[3] The *Lancet*, September 21, 1889.

However, Kelly has said :

> It is hard for a younger surgeon in this day, 1912, to realise how difficult it was for a surgeon in the eighties to emancipate himself from the errors of such masters in surgery as Emmett and Thomas, and to recognise as tubal inflammatory diseases and tubal abscesses, conditions which these great observers diagnosed as phlegmons developing in the para-uterine cellular tissue. The greatest surgical battle ever fought was that waged in the various obstetrical societies on this subject.[1]

It was Tait's surgery that cleared up this mystery. Tait evolved a pathology of the living from his unrivalled experience of tubal disease. He taught this pathology, but he taught to deaf ears ; then he produced his specimens and gradually he convinced the unwilling.

Howard Kelly would willingly steal Tait's thunder if he could, and award to Price the merit of creating pelvic surgery, for he says :

> One can more readily appreciate the pre-eminent and original part Joseph Price has borne in the evolution of the treatment of pelvic inflammatory diseases in the eighties, as he dictated the practice not only of this country, but to some extent of the entire world, when we recall the fact that Tait's work on pelvic abscesses began as late as 1880.

But Price would have nothing to do with this kind of praise, and he, an honest man, said boldly : " In pelvic surgery Tait stood first—taught us the best we know." The man that should share with Tait the honour of advancing the surgery of the pus tube is the German, Hegar ; he it was tried to do for Germany [2] what Tait did for England and America in the surgery of pelvic inflammatory lesions. Joseph Price was a pupil of Tait, and he preached Tait's views to American surgeons, and by his splendid success became the chief American representative of Tait's pelvic surgery ; to Price, then, Tait owes much for making his views known to American surgeons.

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[1] " A Cyclopedia of American Medical Biography," Howard Kelly, 1912, vol. i. p. xlvii.

[2] When we visited many German clinics in 1892 we often read on the case card, in describing the operation performed, " the Lawson Tait operation," never the Hegar operation.

XIX

The International Medical Congress

1881

In August, 1881, the seventh meeting of the International Medical Congress was held in London. The Prince of Wales formally opened the Congress in St. James's Hall in the presence of at least 3000 members of Congress.

The first Congress had been held in Paris, and the subsequent ones at Vienna, Brussels, Florence, and Amsterdam, and at none of these places had the attendance been large, nor had the meetings been a success.

At the sixth Congress, which took place in Amsterdam in 1878, it was decided to hold the next triennial meeting in England in 1881. A provisional committee was formed, and the choice of President fell on Sir James Paget. Lister, who had been living in London for four years, was invited to act as honorary secretary-general; on consideration, however, he determined to decline the task, and the committee invited Mr. MacCormac to fill the post, and he accepted.

Paget in his own quiet but thorough way was determined that the London meeting should be a success, and his efforts and predictions were amply fulfilled.

On the opening day, August 3, 1881, Paget delivered his Inaugural Address before an audience, the like of which, as MacCormac has said,[1] had never been seen before. Over three thousand medical men were present, and of them as many as 1,097 came from America, France, Germany, and all the other countries of Europe, the Colonies, and India. In the audience could be noticed Pasteur, Virchow, Koch, Langenbeck, Charcot, Volckmann, and many other celebrities.

[1] Stephen Paget, "Memoirs and Letters of Sir James Paget," London, 1902.

> Truly it was a great assembly; and Sir James Paget presided over it as probably no other man in our profession could have done. I need not characterise the beauty of diction and harmony of phrase, or the grace and eloquence of the remarkable oration that he delivered.
>
> It was simply perfect, from whatever point of view it might be regarded; and although he accomplished much further work during his well-filled life, yet in his Presidency over this remarkable Congress, the greatest and most important, and I may add the most successful meeting of the International Medical Congress, which has ever taken place, Sir James Paget may, I think, be regarded as having attained the zenith of his fame; and he received without stint the sympathetic admiration of the world of medicine (MacCormac).

It was at this Congress that Robert Koch demonstrated in King's College his then new method of cultivating microbes upon solid media. The illustrious veteran Pasteur was present at the demonstration, and at its conclusion exclaimed, "C'est un grand progrès, monsieur." How vast have been the extensions of our knowledge which have resulted from that great step in advance.

At the Sectional Meeting devoted to Surgery, Tait read a paper on "Recent Advance in Abdominal Surgery," following Spencer Wells, who contributed a paper on "Recent Advances in the Surgical Treatment of Intra-peritoneal Tumours"; while in the Section of Obstetric Medicine and Surgery, Tait took part in the debates on the chief paper which was contributed by Battey, who had been Tait's guest at Birmingham.

In this paper Battey gives his experiences, and draws his conclusions from all the operations which he and others had performed on the ovaries.

The paper was entitled "Oophorectomy—Battey's Operation—Spaying—Castration of Women."

Battey's paper was an important contribution from an historical point of view, because it gave the history of the procedure, the indications for removing the ovaries, a description of the operation and its mortality, together with a list of all the operations that had been recorded up to that time.

Battey included in his list Tait's operations (29 in number),

and made the mistake of saying, " on August 1, 1872, Lawson Tait operated at Birmingham, also with fatal result." We know that Tait has said of this operation for hæmorrhage from a myoma that, " the result was a complete success, and I heard of the patient being alive and well in 1874." [1]

After Battey had finished his paper, Thomas Savage, Tait's colleague at the Women's Hospital, Birmingham, read a paper on " Oophorectomy." He said that he had commenced the operation for oophorectomy forty times in all: five times he was unable to do anything on account of adhesions.

In the debate that followed the reading of Battey's paper and that of Savage, well known surgeons, such as Priestly, Knowsley Thornton, Martin of Berlin, and Spencer Wells, joined in.

Priestly mentioned that in his earlier life, ovariotomy was considered quite unwarrantable, and one authority had proposed that the operator should be indicted for manslaughter. He considered that when all other means had failed that Battey's operation might be tried.

Knowsley Thornton said his own experience of the operation had been small and the results very various, and so he was left in a doubting condition. He pointed out that Battey made out the mortality of the operation to be 18 per cent., while Savage said that its safety was completely established.

Martin, of Berlin, considered that the removal of the ovaries should be undertaken but very rarely. He had performed the operation fourteen times; mostly to check the hæmorrhage due to bleeding fibroids.

Tait then spoke:

> I must express my gratitude to Dr. Priestly for the extremely just and logical manner in which he has stated both sides of this most difficult question. Dr. Priestly alluded to confession of failure in many instances; but this would have to be applied to all kinds of surgical practice if applied here. When a surgeon undertakes the treatment of a diseased joint, the moment he entertains the question of excision of the joint, or when amputation is performed, he not only confesses a failure,

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[1] Tait, " Diseases of the Ovaries," 4th edit. p. 326.

but he confesses one which involves serious mutilation of the patient—a mutilation sending him halting through life; whilst removal of diseased ovaries does not in any way interfere with the patient's personal appearance, or with ability to engage in any kind of active business or amusement; but if successful, it completely relieves her of terrible suffering, perhaps may save her life.

It is worthy of notice that the critics of the operation have subjected it to the crucible of statistical examination, through which they dare not pass their own work. Those who speak of normal ovariotomy, and who are under the impression that in their cases they can with as much ease remove the ovaries of a woman as a sow-gelder can remove the organs of a young pig, are under a delusion, and evidently know nothing practically of the work. It is almost in all instances a difficult operation, sometimes one of exceeding difficulty. The percentage of incomplete cases is by no means a heavy one, for if the records of any other operation in surgery had been as completely kept as had those of the one now under discussion, it is perfectly certain that in many branches a very much higher percentage of incomplete operations would have to be confessed. . . . It must be noticed that the proportion of incomplete operations in removal of ovaries is, in my own experience, rapidly diminishing; for I now can and do remove ovaries which a year or two ago I should have left unremoved. A statement has been made that the ultimate results of such cases have not been published. To such a remark I would say that it is the fault of the speaker, and due entirely to his ignorance of the literature of the subject. I submit a long list of seventy cases.

Tait was followed by Bantock, of London, who said that while he had seen several cases, which could be classed under the neuroses, in which the removal of the ovaries appeared to be called for, yet he had never been able to bring himself into that frame of mind in which he could urge the patient to submit to it. But on a very different footing stood the operation of oophorectomy for the arrest or relief of hæmorrhage.

Spencer Wells spoke, and said that his experience of Battey's operation was very small, as he had performed it *once*. He could not help remarking on the frequency with which certain surgeons thought it necessary to extirpate the ovaries; he didn't see such cases himself; perhaps they all went to Birmingham.

In the report of the debate published by the *Medical Times and Gazette* (August 13, 1881), there is the following paragraph:

> It is indeed an astonishing thing that in a provincial town, two surgeons should within ten years meet with more than one hundred cases in which this operation was absolutely necessary, while in the same time the greatest living authority on diseases of the ovary—a man of world-wide fame—found so few cases in which he could advise it that they could be counted on the fingers of one hand. To give another illustration of the same thing, Dr. Battey, the introducer of the operation, to whom persons go from all parts for his opinion as to its advisability, has in *nine years* met with sixteen cases in which he could advise it. But in Birmingham a large number have presented themselves in as many months in the practice of each of two medical men.

The same reporter gives the following account of Tait's remarks at the meeting:

> Mr. Tait then rose, he distributed a printed sheet [1] containing a list of cases he had operated upon, and successfully appealed to some gentlemen in the audience (who had seen some of his cases) to confirm the truth of the statements made concerning these cases on the sheet. In the sheet, however, the result of the cases was only indicated by words such as "complete cure," "complete relief,"—terms, the meaning of which may, in the minds of different persons, differ, and will, in the first instance, differ according to what may have been expected from the operation. Results so stated will be entirely conclusive arguments only if we are sure that records have been accurately and completely kept, and if we thoroughly understand the meaning attached to the language used. That at least one of the conditions has not been fully complied with, was shown by an explanation Mr. Tait had to make. He had done two different operations on the same day: one patient died, and the other recovered. He had forgotten which one it was that died, and which got on well, and so had erroneously published a case as fatal which was one of cure. It is clear from this that the records upon which Mr. Tait bases his statements cannot have been very carefully kept. Certain criticisms imputing a resort to the

[1] This printed sheet is not reproduced in the *Transactions* of the Congress, nor in any of the medical papers that we have consulted. In the fourth edition of the "Diseases of the Ovaries" there are a series of cases given, and we have no doubt that these tables are the extended form of the tables that were handed out as mentioned above.

operation from pecuniary motives he answered very effectively.[1]

Tait a few weeks later, in a letter to the *Medical Times*, replied to this criticism.

> The criticism your reporter makes concerning the mistake in my having published a case as fatal when it turned out that the patient recovered and was cured, is a little ungenerous. I should have deserved what he said if the mistake had been the other way, and I think it would have been fairer if he had stated exactly what I said—that the mistake was not my own. Even if it had been, the explanation is simple. Nine years ago I had no idea that my practice in abdominal surgery would ever reach the proportion it now has, and at that time I was not in the habit of using a special note-book for each case, as all ovariotomists now do. The fact is, our critics submit us to an ordeal in the way of statistical investigation, to which they themselves cannot, will not, or dare not, submit their own work. Such a mistake as this by no means involves your conclusion that these cases "cannot have been very carefully kept."

When we glance through the papers of Battey and Savage, and read through the remarks made by Priestly, Wells, and Thornton, who represented England, and see what Martin, of Berlin, had to say, we realise that there was a strong suspicion of doubt about this operation. Martin, Thornton, and Bantock were quite willing to admit that experience had shown in their own hands, and in the hands of Hegar, that the removal of the ovaries had in some cases been followed by a cessation of hæmorrhage, but they were all doubtful concerning the operation when it had been performed for dysmenorrhœa, insanity, epilepsy, and causes classed conveniently under the wide heading "neuroses." We must admit that their surgical instinct was correct, and Mathews Duncan, with his fine Scotch common sense, was not slow to express this feeling of distrust on the supposed proper indication for this operation.

The debate is interesting now because the historian of pelvic surgery can affirm that the Fallopian tubes do not appear to have been mentioned; yet Tait's list of seventy

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[1] *Med. Times and Gaz.*, September 17, 1881.

cases most assuredly embraced his operations on the diseased tubes; nevertheless he does not appear to have realised that he, of all the men assembled at that meeting, could have struck the keynote, and could have pointed out that the reason why they were all groping in the dark for the truth, was because they all took the ovary as the essential factor, and none of them realised, not even Tait himself, that the Fallopian tubes were in reality the organs which were the real cause of much of the sufferings attributed to the ovaries. The next few years, however, taught Tait the truth, and from this time onward he became the apostle who preached the gospel concerning salpingitis, and his work made him the founder of pelvic surgery, from which developed abdominal surgery.

Tait's paper in the Surgical Section was very short, but it was of the greatest importance. Any paper which related the first successful cholecystotomy performed in Europe, which dealt with the drainageof cystic kidneys, which touched on the surgery of the spleen, and related the first attempt to cope systematically with pelvic suppuration by abdominal section, which recorded five successful operations in dealing with ectopic pregnancies which had gone to term, and which gave an account of operations on Fallopian tubes filled with serum, blood, or purpulent fluid, and which concluded with an account of the successful treatment of uterine myoma by removal of the uterine appendages: any such paper read by the man who performed all these original operations must be considered important: in fact, the importance of the paper could not then be realised. One knows now what a truly wonderful paper this was, and as the years have passed all but one of these operations, first advocated by Tait, have become the routine work of everyday surgery.

In the discussion that followed the reading of the various papers, Marion Sims said:

"The papers read mark an era in the progress of surgery, and will lay the foundation for still greater results. Mr. Lawson Tait's paper is hardly open to criticism, or discussion. We have simply to accept the teachings of his experience as he has presented them."

Two years later, when Tait published the fourth edition of the "Diseases of the Ovaries," he included this paper in the end of the volume, and it is very wonderful to see in the few years that intervened between the reading of the paper at the Congress and the publication of the book, how he had worked along the lines that he had laid down, and how marvellous were the results that he was able to show in such a short time. He was breaking new ground in unexplored fields, and the great ocean of truth lay before him. When he had opened up these new fields, he sent forth from time to time an argosy laden with new truth to all the nations of the world. Never in the history of surgery has it fallen to the lot of any other worker to be the originator of so many new operations on so many different organs.

XX

Ectopic Pregnancy

1881–1883

Previous to 1883 no one had attempted to operate on a case of ectopic pregnancy in which a rupture of the tube had recently taken place. With regard to some of Tait's other operations, claims have been put forward by some who held that others have forestalled Tait; but no one has attempted to claim priority in regard to the operative treatment of recent ruptured ectopic pregnancy; Tait has the field to himself.

But it must not be supposed for one moment that cases of ectopic pregnancy which have ruptured, and in which the fœtus had continued to live and grow, had not been operated on. In order, therefore, to show what was previously known, and what achieved, as regards the treatment of such a pathological catastrophe, we must go back as far as the time of Albucasis, in the middle of the eleventh century, who appears to have been the first to describe a case of extra-uterine pregnancy.

Webster [1] in dealing with the historical aspect of this subject says:

> In studying the literature of ectopic gestation two names, viz., those of Dezeimeris and Lawson Tait, stand out in such prominence that they serve to indicate its division into three distinct periods, which may be stated as follows:
>
> 1. The period before Dezeimeris;
> 2. The period between Dezeimeris and Lawson Tait;
> 3. The present period.

The first case that we have mention of, as we have stated above, is one by Albucasis,[2] an Arabian physician living in

[1] "Ectopic Pregnancy," Pentland, Edinburgh, 1895.

[2] Lib. ii. sect. 76. Also "De Chirurgie," cura J. Channing, Oxon., 1778.

Spain in the eleventh century. He observed the parts of a fœtus escaping through the abdominal wall, a sinus having formed as the result of suppuration about the fœtus, which was expelled through the umbilicus.

Secondary abdominal pregnancy was first described by Berengario de Carpi.

Berengarius Carpus, as he is usually styled, was the first of the great anatomists and was the worthy predecessor of Vesalius. In his "Isagogae breves in anatomiam humani carporis," one edition of which was published in Venice in 1535, we have the first description of abdominal pregnancy; which was generally considered to result from rupture of the gravid uterus, the ovum escaping into the peritoneal cavity.

During this century (the sixteenth), several cases of ectopic gestation were described, but so vaguely that their exact nature cannot be determined: among these may be specially mentioned the cases of Polinus,[1] Horstius,[2] Platerus,[3] and Primerose.[4]

During the seventeenth century more exact descriptions were published, and distinct varieties were mentioned.

In Bauhine's Latin translation of Rousset, 1601, p. 294, there is a good history of a fœtus that had remained within the abdomen twenty-eight years, and had been converted into a hard mass. The patient died in 1582, at sixty-eight.

Riolanus,[5] one of the most celebrated of the old French anatomists (born 1577), examined a case of tubal pregnancy and described it in his *Anatomia, seu anthropographia* (Paris, 1626). This is the first accurately recorded case of the kind.

Riolanus mentions that in 1614 Mercerus examined the body of a woman who had died of a ruptured two months' ectopic pregnancy; he thought that it was a right ovarian

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[1] "Miscell. Nat. Curios," 1670. Obs. 110.
[2] "Opera Medica," Norimb., 1660.
[3] "De partium corporis humani structura et usu," Basil, 1597.
[4] "De mulierum morbis et symptomatis," lib. iv., Rotterodami, 1655.
[5] This is the Riolanus who refused to accept Harvey's ideas about the circulation of the blood; and he is the Riolanus that Huxley styled "a tympanitic Philistine, who would have been none the worse for a few sharp incisions."

pregnancy, but he mentions that the tube was much enlarged and inflamed. Later on, St. Maurice [1] described what he thought to be another instance of ovarian pregnancy.

Webster says that it was in the seventeenth century that a clear distinction was recognised between primary and secondary abdominal gestation: one of the earliest cases of the former variety being mentioned by the Abbe de la Roque [2] in 1663; others are noted by De Monconys [3] and Courtial.[4]

During the eighteenth century cases were noted by Martin, Duverney, Turnbull, and others; while the occasional occurrence of uterine, along with extra-uterine, pregnancy has been known since the days of Albucasis.

The first description of *Interstitial Tubal Pregnancy* [5] in which the pregnancy developed in the uterine portion of the tube, was given by Dionis of France in 1718, though Mauriceau had previously recognised this condition.

In the nineteenth century Schmidt, of Vienna, was the first to publish a description of interstitial pregnancy in the first volume of "Memoirs of the Josephine Academy" (Vienna, 1801).

Similar cases were described by Dance, Hedrich, and others, all of which were considered by Breschet (1824); while Carus, in the fifteenth volume of Busch's "Neue Zeitschrift fur Geburtskunds," Berlin, 1844, collected the histories of fifteen cases of interstitial pregnancy.

The first clearly described case of extra-peritoneal development of the ovum between the layers of the broad ligament was that of Madame Lefort, published by Bergeret, towards the end of the eighteenth century.

Loschge, of Erlangen, in 1818, and Lobstein, of Strasburg,

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[1] J. J. Mangetus, "Theatrum Anatomicum," Geneva, 1717, t. ii. p. 140.
[2] "Ephemeri des med. Gallicæ," Paris, 1663.
[3] "Itiner. Italic," t. ii.
[4] "Nouvelles observations," Leyde, 1709.
[5] In Granville's "Graphic Illustrations of Abortion" (London, 1834) there is given on Plate X. Fig. 1, a specimen entitled *ovum embryoperum in receptaculo interstitiale.* This is clearly a specimen of interstitial pregnancy, or as it was called by Ramsbotham ("Obstetric Medicine,' plate 88), *parietal extra-uterine pregnancy.*

in 1824, described other cases; but it was Dezeimeris,[1] however, who first gave the name of *sub-peritoneo-pelvic* to this variety, and pointed out its importance.

Leaving for the present the various methods of classification adopted by Bianchi, Josephi, and others, until we come to deal with Tait's classification, we shall now look into the history of the medical and surgical treatment of ectopic pregnancy which has been worked out by Parry,[2] Strahan,[3] and Webster.

Before the date of Tait's first classical operation in 1883, the great risk to life to the woman in whom an ectopic pregnancy had been diagnosed was fully recognised, and the treatment suggested and carried out aimed at preserving the life of the mother and sacrificing the fœtus.

This being the first aim of treatment, we are not surprised to find that the most extraordinary means were suggested for the destruction of the ovum by influences brought to bear on the mother, or directly on the gestation sac.

Thus at various times attempts have been made to weaken the mother through restricting her diet, bleeding (Cazeaux) and purging her, and she was dosed with iodide of potassium (Keller) and mercury (Nureau). Strychnine (Barnes) was administered in the hope that it would kill the fœtus; while it was even suggested that mothers should be syphilised in the hope that the fœtus would die.

Others proposed that the *liquor amnii* should be drawn off from the gestation sac. Baudelocque and Quelin, according to Baart de la Faille, were the first to suggest this plan, while Delisse and Kiner actually carried it out in 1818. Kiwisch claims that Basedow first recommended it. The plan has been carried out not only for the destruction of the fœtus, but in some cases puncture has been made, either through the vagina or abdomen, for diagnostic purposes.[4]

[1] "Grossesses extra-utérines," *Jour. d. conn. méd. chir.*, Paris, December, 1836, p. 257.

[2] "Extra-uterine Pregnancy," London, 1876.

[3] "The Diagnosis and Treatment of Extra-uterine Pregnancy," Phila., 1889.

[4] Jonathan Hutchinson relates a case (The *Lancet*, July 19, 1873) where he performed paracentesis and removed six pints of fluid. The trocar puncture never healed, and two weeks later a lock of fœtal hair presented and thus cleared up the diagnosis.

In place of paracentesis others suggested that drugs should be injected into the gestation sac in order to destroy the fœtus.

Joulin first proposed this in 1864, and the method was carried out by Friedrich in 1864. It has been employed by others, and the drug used has been morphia, and while the fœtus has been destroyed in some cases and the ectopic mass has remained quiescent, in other cases hæmorrhage and septicæmia have resulted.

It was proposed that the gestation sac should be compressed by bags of sand placed on the patient's abdomen; while in other cases the destruction of the ovum was carried out by the passage of an electric current through the gestation sac. This was successfully done by Bachelli, of Pisa, in 1853.

When we come to the treatment of ectopic pregnancy by gastrotomy, we find that the first cases treated were instances where the patient had passed through the crisis of rupture of the tube and the fœtus had continued to live and grow outside the tube.

Cornax, in the middle of the sixteenth century, is the first who is said to have performed an abdominal section for this condition, while Abraham Caprianus is said to have performed it at Louvade in 1694. These appear to have been instances where the cyst has contracted adhesions with the abdominal wall, after which a fistula has formed, then the surgeon has enlarged the opening and allowed the fœtus to be discharged.

It is to Primerose [1] that the credit of performing the first abdominal section for this condition must be given. He operated in 1594. Parry says: the history of this patient has become classical. She was twice pregnant with extra-uterine children—first in 1591, and again some time before 1594. The cyst of the first child opened spontaneously through the abdominal wall. "The fistula was enlarged, and this child extracted by Jacob Noierus, a surgeon. This operation proving successful, Primerose removed the second infant by gastrotomy two months later."

[1] "De Mulierum morbis et symptomatis," lib. iv. p. 316.

Felix Platerus [1] reported another successful case three years after the case of Primerose. After this there is no mention of any further operation for at least a century, then we find that Calvo [2] reports a case in France in 1714, and in 1759 John Bard,[3] of New York, made a diagnosis of extra-uterine pregnancy, and opened the abdomen and removed a macerated fœtus: the wound was kept open and the patient recovered.

About this period (1756), Kelly [4] advocated opening the gestation sac through the vagina and removing the fœtus. Campbell says that this was actually done as early as 1722.

In 1791, Baynham, a country physician in America, performed an abdominal section and removed a fœtus successfully from the wife of a Virginia planter, and in 1799 he operated with the same happy result on a negro slave.[5]

The operation was performed by other American surgeons, viz. McKnight (1795), Wishart (1825), and later on in 1846 by Stevens of New York.

During the nineteenth century numerous cases of ectopic pregnancy which had reached, or had passed, term were operated on. Writers on obstetrics were fully alive to the course that was followed when a tube ruptured and the fœtus continued to live. They knew only too well that if the fœtus continued to live and grow that any operative procedure was attended with great risk, for the intense vascularity of the placenta constituted at once the most outstanding feature, and the most dangerous characteristic of these cases. After the fœtus had died there was at once a striking diminution in vascularity, which made the case more amenable to operative treatment.

That these cases are far from common is shown by Parry's lists. He published his splendid work on "Extra-uterine

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[1] "De partum Corporis Humani, Structura et Usa," 1597.
[2] "Hist. de Acad. Roy. des Sciences," 1714, p. 29.
[3] "Med. Obser. and Inquiries," London, 1764, vol. ii. p. 369.
[4] "Med. Obser. Soc. Phys.," London, 1797, p. 44.
[5] Parry very justly remarks, "Dr. Baynham's cases are well worth attentive study. They illustrate the intrepidity and good judgment so often displayed by the provincial surgeon, who, separated by long distances from his fellows, often has to act in the greatest emergency without the counsel which he may earnestly desire."

Pregnancy" in 1876, and he based his deduction on an analysis of 500 reported cases.

He gives a list of sixty-two gastrotomies for the removal of extra-uterine children, and the list shows that while nearly all the children removed died, yet many of the mothers recovered. The actual figures were sixty-two cases, of these thirty lived and thirty-two died.

During all this period (from 1800 to 1870) the question that had continually cropped up was, "Is it not safer to operate by the vaginal route?"

Tait himself operated on one case by vaginal section, but the patient died a few hours after the operation. This operation was performed in 1872, and he was so dissatisfied with the method, and the result, that he abandoned it at once, and never was in favour of it afterwards. Henceforth, he attacked all his cases by abdominal section, and with excellent results.

These cases lead up to his case in 1883 when he decided to operate soon after the tube had ruptured, and not only did the new operation save many lives by arresting the hæmorrhage incidental to the rupture of the tube, but it was the means of removing those fœtūs which were yet alive and which might, under favourable circumstances, go on developing till they had arrived at term, and would have to be dealt with then at great risk to the mother.

Previous to Tait's time the suggestion had already been made that the ruptured tube should be operated on with the idea of arresting the hæmorrhage, and saving the patient. Parry's chapter, written in 1876, shows what an absolutely neglected field of surgery this was. He says:

> In speaking of the results of this pitiless termination of extra-uterine gestation, it was stated that so few recover from it, that all hope of such a happy result is to be dismissed in considering the treatment. No doubt, notwithstanding the statement of Rogers to the contrary, a few women have recovered, though the number is very small, so small that when one is called to a case of this kind, it is his duty to look upon his unhappy patient as inevitably doomed to die, unless he can by some active measures wrest her from the grave already yawning before her. The history of human injury

and disease presents no conditions parallel to this one. However fatal the disorder, science and art have found some means of prolonging life or soothing the stormy passage to the grave.

A bleeding vessel, through which the red stream of life is rushing away, can be ligatured. A gangrenous limb, which is destroying its possessor by sending its poisonous emanations to the remote regions of the body, can be amputated. A cancerous breast, which is sapping the vitality of its victim hour by hour, can be removed with the prospect of temporary relief. An aneurism, that places life in constant jeopardy, can often be cured by proximal or distal ligature. The tumultuous action of a heart organically diseased may be quelled till nature restores the balance, after which the person may enjoy a long and even a useful life. Even phthisis now counts its many cures; but here is an accident which may happen to any wife in the most useful period of her existence, which good authorities have said is never cured; and for which, even in this age, when science and art boast of such high attainments, no remedy either medical or surgical has been tried with a single success. From the middle of the eleventh century, when Albucasis described the first known case of extra-uterine pregnancy, men have doubtless watched the life ebb rapidly from the pale victim of the accident as the torrent of blood is poured into the abdominal cavity, but have never raised a hand to help her. Surely this is an anomaly, and it has no parallel in the whole history of human injuries. The fact seems incredible, for if one life is saved by active interference it may be triumphantly pointed to as the first and only instance of the kind on record. In the whole domain of surgery—for we cannot look to other than surgical measures under the circumstances—there is certain death. . . . The only remedy that can be proposed to rescue a woman under the unfortunate circumstance is gastrotomy—to open the abdomen, tie the bleeding vessels, or to remove the sac entirely. This treatment was suggested by Dr. W. W. Harbert in 1849, and again by Dr. Stephen Rogers of New York, in a paper read before the American Medical Association in 1866. In 1853, N. Alexis Moreau emphatically condemned gastrotomy under these circumstances, because he believed that the hæmorrhage could not be arrested.

It is, indeed, to Rogers of New York that the credit must be given of formulating the arguments in favour of performing abdominal section to save a woman dying from early rupture of a pregnant tube; and after him Meadows, Hewitt, Greenhalgh, Playfair, Koeberlé, Behier, Schroeder, and Atlee countenanced the proceeding, but none of these

actually did the operation. To Tait belongs all the honour.

At a meeting of the Obstetrical Society of London, April, 1863, Oldham, the president, said that he was anxious to direct the attention of the Society to the possibility of performing gastrotomy in some cases of tubal pregnancy, where death was about to result from internal hæmorrhage —the danger was so great that it justified any expedient which might have a hope of success.

Howard Kelly [1] writing about the operation says: "The most dramatic operation in the whole realm of surgery is that for a ruptured extra-uterine pregnancy, which lingered so long as one of the most hopeless and distressing ailments. Conquest of this malady was one of the most brilliant achievements of the last century."

Tait has left us an account of the first case of ruptured ectopic pregnancy that he was called to, and he has told us of his regrets for not operating on it. He says: [2]

> In the summer of 1881 I was asked by Mr. Hallwright to see with him, in consultation, a patient who had arrived by train from London in a condition of serious illness, that illness having been diagnosed by Mr. Hallwright as probable hæmorrhage into the peritoneal cavity from a ruptured tubal pregnancy. The patient was blanched and collapsed, the uterus was fixed by a doughy mass in the pelvis, and there was clearly a considerable amount of effusion in the peritoneum, but no distinct tumour could be felt above, and I agreed with Mr. Hallwright as to the nature of the lesion. This gentleman made the bold suggestion that I should open the abdomen and remove the ruptured tube. The suggestion staggered me, and I am ashamed to have to say that I did not receive it favourably. I saw the patient again, in consultation with Mr. Hallwright, and Dr. James Johnson, and again I declined to act upon Mr. Hallwright's request, and a further hæmorrhage killed the patient. A post-mortem examination revealed the perfect accuracy of the diagnosis. I carefully inspected this specimen which was removed, and I found that if I had tied the broad ligament, and removed the ruptured tube I should have completely arrested the hæmorrhage, and I now believe that had I done this the patient's life would have been saved.

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[1] "A Cyclopædia of American Medical Biography," Howard Kelly, 1912.

[2] "Diseases of Women and Abdominal Surgery," 1889, p. 454.

> After the terrible lesson given to me by Mr. Hallwright's case, I did not see another example of ruptured tubal pregnancy, or one which I suspected to be of that nature, till I was called to Wolverhampton by Mr. Spackman, on January 17, 1883. There could be no doubt as to the nature of the case, and Mr. Spackman was fully aware of it before I was summoned. The patient was clearly dying of hæmorrhage, and I at once advised abdominal section. The fœtus, about the twelfth week, was lying amongst masses of clot and coils of intestine, and to these latter the partially extruded placenta had obtained new attachments. These I cautiously separated, and occasioned fast and copious bleeding at every point. I wasted much time in trying to stop this hæmorrhage, so that by the time the operation was finished my patient was practically dead. We got her to bed alive, and this is all that can be said. I thought much about the case, for it was a bitter disappointment: I thought I should achieve a triumph, and I had only a failure. But my conclusion was speedily arrived at, that I had blundered—that the true method of operating on such a case was to separate adhesions rapidly, regardless of bleeding, and make at once for the source of the hæmorrhage, the broad ligament, tie it at its base, and then remove the ovum, debris, and clots at leisure.

Tait soon after this, on March 3, 1883, operated on a case to which he was called by Dr. Page, of Solihull, and he was able to save the patient, and he continued to operate on all cases that he was called to, the result, up to 1888, being forty-two operations, with two deaths; one of these being his first operation, in 1883, and another his twenty-fourth in 1886.

As the years passed on and Tait's experience increased, he gradually evolved his very definite views on the pathology of ectopic pregnancy, many of which are accepted now on all sides: we shall deal with these views in a later chapter.

XXI

Diagnosis and Treatment of Chronic Inflammation of the Uterine Appendages

1882

In July, Tait contributed to the *British Medical Journal*[1] a paper on the "Diagnosis and Treatment of Chronic Inflammation of the Ovary." This is probably the first important paper that he wrote on the subject.

A few weeks before it appeared, Spencer Wells had brought out a work "On Ovarian and Uterine Tumours," and in a chapter devoted to "Recent Extensions in Ovariotomy," he discussed the extirpation of normal ovaries and concluded that he could not speak favourably of the operation; he believed that the operation had a very limited application, even as a remedy for bleeding uterine fibroids. He thought that the operation was so open to abuse, that its introduction in mental and neurotic cases was only to be thought of after long trials of other tentative measures.

Tait fully recognised the fact that Spencer Wells had a very large following, and that his expressed opinion would have great influence on the profession in London and elsewhere. When, therefore, Wells, in his chapter on "Recent Extension in Ovariotomy" deliberately refrained from alluding to the subject of inflammatory diseases of the tubes, he at once made it evident that the subject, in his opinion, was one that was not worthy of consideration; while in his remarks on "Battey's operation," he made it clear that he thought that the operation was one of very limited use.

Tait knew that it was necessary to produce a different impression on the profession if he wanted his new ideas to gain ground, so he contributed a paper on the "Diagnosis and Treatment of Chronic Inflammation of the Ovary" to

[1] *Brit. Med. Jour.*, July 29, 1882.

the *British Medical Journal*, and took the opportunity to remind the profession of Wells' high mortality in ovariotomy; a mortality that had existed during the first seven hundred of Wells' operations. The paper was, no doubt, designed to prepare the minds of the gynæcologists who were likely to be present at the forthcoming meeting of the Obstetrical Society, where Tait proposed to exhibit a number of specimens of diseased Fallopian tubes and ovaries, to demonstrate that the diseases that he had written about did actually exist, and were not mere *post-mortem* curiosities.

In this paper, Tait has left us some interesting observations on the evolution of the treatment of pelvic diseases. He says:

> The diagnosis of pelvic diseases may be said to have received its first real life from Simpson, for before his day no such attention was given to those affections as deserved the name of system and study. He it was who instituted precise means of physical diagnosis, and upon that he reared methods of treatment which have made a lasting impression on our practice. Like all innovations, Simpson's methods led, even in his own hands, but mostly in those of his friends and followers, to an excess of zeal; and the mechanical school of gynæcology, of which he was unquestionably the founder, led many to the belief that, armed with a sound, a speculum, a caustic stick, and some new-fangled pessary, the practitioners could subdue all the pelvic ailments of women. All these aids, valuable in their way, had their enthusiastic supporters, were declaimed against by others, did an infinite amount of mischief in their turn, and have finally been referred to more limited and less hurtful fields of practice.
>
> From the same phase of surgical development arose a number of operative proceedings, each of which has extended our means of relieving human suffering, but each of which went through a course of rough experimentalisation, which is now terrible to look back upon. Simpson found that division of the cervix relieved the sufferings in certain cases of dysmenorrhœa, and enabled a few sterile women to become pregnant. Immediately, we had a flood of hysterotomes all over the world, and every sufferer had her cervix divided. Thousands of wholly unnecessary operations of the kind were, and many still are, performed, and not a few deaths occurred from the practice.
>
> Coincident with the innovation we had the employment of pessaries, fortunately a less harmful practice, but carried to an extent of uselessness which is positively amusing; and other illustrations of similar strivings in the dark might be given.

From the writings of Dr. Henry Bennet and Dr. Tilt, especially the latter, another new departure was derived; for attention was directed, by these authors, to the possibility of the ovaries being the seat of the troubles, relief of which was sought in the treatment of the uterus.

Still another impetus, and the greatest of all, in my opinion, was given to gynæcology by Dr. Thomas Keith, who taught us that our traditional fear of the peritoneum was only a bugbear, and that it would serve us as well as any other part of the body if dealt with fairly. After Mr. Spencer Wells had gone on for twenty years operating on hundreds of cases of ovarian tumours, with a mortality of about twenty-five per cent., Dr. Keith persuaded us that ovariotomy could be done with less than six per cent. of deaths. The mortality of Dr. Keith, and my own, is now as low as three per cent., and this after we have both tried the so-called antiseptic system of Lister, and have given it up as more dangerous than useful.

The outcome of such splendid work in the removal of ovarian tumours will soon be felt in very many ways, but in one it has already given evidence of another and altogether new direction for abdominal surgery. As long as Mr. Spencer Wells' example ruled our practice, and as long as his high death-rate was the best we could get, we operated on ovarian tumours only when they threatened life, and we delayed the case by mischievous tappings as long as we could. We felt that we were not justified in opening the abdomen for conditions whose severity did not threaten life. Now, however, when the removal of an ovarian tumour is fatal only when the patient has been tapped, or the operation injudiciously delayed, we are justified in performing abdominal section not merely for the saving of life, but for the relief of suffering.

This new practice has had many good results, not the least of which is, that it is shedding a whole flood of light on the pathology of pelvic diseases, and is even helping us to understand the physiology of the female sexual organs. Thus my own practice, the details of which will shortly appear in a special work, has convinced me that the usually accepted doctrine of the coincidence of ovulation and menstruation is wholly erroneous. The ovaries have nothing to do with menstruation; and though I give this opinion with a qualification which may be made necessary by further experience, so far the evidence before me is convincing that the phenomenon of menstruation depends upon the Fallopian tubes, and not upon the ovaries. Finally, and of most importance for my present purpose, we are for the first time becoming cognisant of the real conditions involved in the diseases recognised as chronic inflammation of the ovaries. For such a purpose as this, an abdominal section has been well said

to be as much value as a post-mortem examination ; I would say it is more valuable, if we have the recent and exact record side by side with the morbid appearance.[1]

Tait then proceeds to state the origin of the acute diseases of the ovaries, and paints the life of misery that a woman leads who suffers with inflamed ovaries. He gives her symptoms, and then points out what the finger of the properly educated gynæcologist will discover on examination : " a fixed and leaden mass, comprised of the enlarged and probably adherent ovary, or of the occluded and distended tube, will be felt on one or both sides of the uterus, through the vaginal cul-de-sac ; and the peculiar sickening pain felt by the patient when the mass is touched will afford conclusive proof as to its nature."

Tait then asks the question, " What is to be done to relieve such cases ? " He answers that rest is to be tried ; counter-irritation by blisters, and the administration of bromides. He then proceeds : " After a persistent trial of them without benefit, there remains for consideration the question of removal of the diseased organs—a proceeding which is based on the soundest and most completely accepted rules of surgery ; " and he proceeds to justify the operation by quoting his own small death-rate and the results recently published in Agnew's " Surgery," and concludes the paper by quoting two of his own cases.

We, who now know so much more about salpingitis, would look upon Tait's paper if published now as superficial, but when we consider that the paper was written nearly forty years ago, and we know that what Tait said was quite true and quite correct, we must accord him our admiration for having the clinical knowledge which others did not possess, and for being able to point out what only a few before him had recognised, that the tender masses found in the pelvis during a vaginal examination consisted not only of the inflamed ovaries, but " of the occluded and distended tubes," and Tait's work for some years to come

[1] This idea was afterwards made use of by Moynihan in his " Pathology of the Living," Saunders, 1910.

was largely devoted to teaching others the truth of this observation.

In order to convince his hearers of the truth of what he said, he found it necessary to exhibit the specimens that he was now continually removing from patients sent to him from all parts of England and from America.

Accordingly, on December 6, 1881, Tait brought forward at the Pathological Society a series of cases, in which he had removed the uterine appendages on account of the occlusion of the Fallopian tubes and their distension by fluid, either serous or purulent. The specimens were exhibited, and were afterwards placed in the Pathological Series of the Hunterian Museum. Tait at this meeting said that during the operation the organs were nearly always found to be matted to the pelvic wall and to the viscera, and the hæmorrhage was often severe during the separation of the adhesions. All the operations of this character that he had performed, nineteen in number, had recovered completely.

A little later, at the July meeting of the Obstetrical Society, 1882, he again exhibited specimens of hydrosalpinx and pyosalpinx from eighteen cases.

Before this meeting took place, Tait wrote a letter to the *British Medical Journal*[1] on "Removal of Uterine Appendages," in which he says:

> As I hope to be able to lay my experience upon this subject before the Obstetrical Society early next session, and partially before the Obstetrical Section at Worcester, I desire now to express a hope that the eminent obstetrical physicians, who have expressed strong disapproval of such operations, will take advantage of one or other of these opportunities to meet me and discuss the question fully. At the Royal Medical and Chirurgical Society, at the Pathological Society, and at various meetings of less importance, I have given already full and frequent opportunities for criticism of all my cases, and no one has yet appeared to do battle; yet, by private report and newspaper innuendo, these operations are doubted, disputed, and abused. Will the critics once for all accept my challenge.

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[1] July 22, 1882.

Tait then says that he wishes to object to the terms "Oophorectomy," "Spaying of women," and "Battey's operation," and concludes with, "May I therefore plead that the operation shall be termed 'Removal of the Uterine Appendages,' and that the classification of the cases shall be based on the morbid conditions found?"

In his remarks at the Pathological Society, Tait said that the nature of the contents of the tube is determined by causes which he did not as yet understand.

In his communication to the Obstetrical Society he is more certain of the cause, for he says: "In one case the pyosalpinx was of an acute character, the immediate result of gonorrhœa, and the inflammation had extended into the general peritoneal cavity, which contained pus. In four other cases the pysosalpinx was clearly to be traced to gonorrhœal infection."

Tait said:

> that he exhibited the specimens to show that the criticism offered against him of removing uterine appendages unnecessarily was altogether unfounded. No other treatment had been, or could be, of the slightest benefit. Immediate and complete relief was obtained in most of the cases, and in those where this was not the case the improvement was slower, but satisfactory, even when it extended over some months. There was no mortality from the operation at all, though it was more difficult than any ordinary ovariotomy, the trouble from adhesions and hæmorrhage being often very great.

Tait then went on to say, that he objected to the operation being termed "Spaying," or "Castration of Women," or "Normal Ovariotomy." All these names were very unfortunate, as they conveyed the idea that healthy appendages were removed from healthy women. He had, however, never performed such an operation. He equally objected to the term "Oophorectomy," as, in the great majority of the cases, it was the *tubes*, and not the ovaries, which were at fault; the removal of the ovaries being then a mere incident in the proceeding.

He then continues:

> The critics of the operation alleged that unnecessary operations were performed. This might be so, though he

did not think so. If so, it was the inevitable result of imperfect knowledge in the early stage of every medical and surgical proceeding. But there was another moral point of far greater importance which the critics forgot. There were hundreds of suffering women to whom removal of the uterine appendages offered a certain and the only method of relief, and the misrepresentations to which the operation was being subjected would prevent some, who were sensitive to criticism, from doing their duty to their suffering patients.

Tait said that he regarded the operation in the neurasthenic class of cases as being of very doubtful value; but for the relief of the symptoms of myoma, he found that the operation, in his hands, now had a mortality less than that of lithotrity in the male, so far as he could ascertain that mortality, and its after results were more certain than those of crushing stone in the bladder. For the third class of cases, those from which he had taken the appendages exhibited, it was the only treatment which offered any hope of relief.

Tait, as we have mentioned, found at this period that it was absolutely necessary to carry his specimens from one meeting to another and exhibit them, to convince his hearers that what he said about diseased tubes was a reality. He took the trouble to go to Ireland and take specimens of pyosalpinx and show them at the Surgical Society of Ireland, and he then presented them to the Royal College of Ireland.

At the International Congress, when Spencer Wells spoke in the discussion on "Battey's operation," he made use of the words, which Tait often used against him in after years, that while some surgeons found it necessary to extirpate the ovaries, "*he never saw such cases; perhaps they all went to Birmingham.*" Spencer Wells but voiced the opinions of many other gynæcologists of the day who did not really believe that these small diseased ovaries existed, and were quite in the dark concerning diseased conditions of the Fallopian tubes.

Alban Doran has recorded his astonishment, when working at the Pathological Series in the Museum of the Royal College of Surgeons about this period, to find specimen after specimen arriving at Lincoln's Inn Fields from Birmingham, and he says, "I was astonished to find how salpingitis

and allied affections had been overlooked by the pathologists and the surgeons." [1]

This want of knowledge concerning Fallopian tubes does indeed seem incredible. There may be some excuse for a complete ignorance about the very early observations on salpingitis, but considering that during the nineteenth century dilated tubes had not only been described, but figured in many well-known works, there can be no excuses for the attitude that the profession took up with regard to Tait's statements concerning diseased tubes; and it was not until he had demonstrated his specimens at meetings in London, and in all parts of England, that his facts were very grudgingly admitted.

Tait himself has left us some account of the early knowledge of the pathology of the Fallopian tubes.

Before giving his observation, we should like to draw attention to the fact that an attentive study of the works of Rufus, Galen, and Ætius, shows us that they were acquainted with the Fallopian tubes, while Herophilus and Soranus did not know of their existence. Furthermore, the first-mentioned authors understood that these tubes were the means of communication between the ovaries and the uterus, and they considered that the normal secretion of the Fallopian tubes was the substance secreted by the ovaries.

The description given by Fallopius (1561) is most accurate and graphic and deserves to be quoted. He says:

> That slender and narrow seminal duct rises, fibrous and pale, from the horn of the uterus itself; becomes, when it has gone a little bit away, appreciably broader, and curls like a branch (capreolus) until it comes near the end; then losing the hornlike curl, and becoming very broad, has a distinct extremity which appears fibrous and fleshy through its red colour, and its end is torn and ragged like the fringe of well-worn garments, and it has a wide orifice which lies always closed through the ends of the fringe falling together; and if these be carefully separated and opened out, they resemble the orifice of a brass trumpet. Wherefore, since the seminal duct from its beginning to its end has a likeness to the bent

[1] *Trans. Obst. Soc.*, London, vol. xlii. p. 61.

part (capreoli) of this classic instrument, separate or attached, therefore, it has been called by me the *uteri tuba*.[1]

To Fallopius, therefore, we owe the term *tube*.

Let us see what Tait found with regard to the early knowledge regarding the pathology of the tubes. He says:

> The first discussion of the pathology of the Fallopian tubes which I have been able to trace is the observation of Friedrich Ruysch in 1665, that occlusion of the tubes is a cause of sterility —a fact which many, who have taken part in recent discussions, altogether overlook when they talk of unsexing a woman by removing diseased Fallopian tubes. Josias Weitbrecht, the great anatomist of St. Petersburg, in his anatomical observations (1742), describes the obliteration of the tubes in a woman who had had a child and then remained sterile. It was, in fact, just such a case as we find commonly enough to be the result of puerperal perimetritis. John Henry Brechtfeldt, of Copenhagen (1675), describes a case of dropsy of the tube which he cured by paracentesis, the woman having a successful accouchement afterwards; but in this case the certainty of Brechtfeldt's diagnosis is open to doubt.[2]

Dr. Macardle, of Washington, D.C., favoured Tait with the result of his researches in the same line, and Tait says: "He has found that Nicolaus Tulpius (obs. 44, lib. iv., 'Observationes Medicorum,' Amstel, 1652) relates the case of Cathalina Bonevallier who died from 'hydrops in cornubis uteri.' The disease is also discussed by Mey ('Hydrops uteri tubarum'). In a Miscellania Curiosa (Nuremberg, 1698) a fatal case of dropsy of the right tube in a virgin is described as having taken place in 1660, and another on p. 232 of the same work. John Baptist Branchi, in his 'De Naturali et Vitiosa Generatione' (Turin, 1741), p. 187, gives an account of a case where the tube and its contents weighed eighty pounds, and Mummicks ('Bibliotheca Anatomica,' vol. vi. p. 624, Utrecht, 1697) relates a case of hydrosalpinx where the cyst and the contained fluid weighed 112 lbs." Tait says:

> I think there can be no doubt that these two cases were really cases of parovian cysts, with the tubes drawn over

[1] Stewart McKay, "History of Ancient Gynæcology," p. 246.
[2] "Diseases of Women and Abdominal Surgery," 1889, p. 335.

them and hypertrophied, as we often see them. We can easily imagine a seventeenth-century observer doing as I have known one in the nineteenth century doing—mistaking such a cyst for an ovary or a tube distended. . . .

It was not, however, till Astruc wrote (1761–66) that any serious attention seems to have been given to diseases of the Fallopian tube, and it is not a little curious that what he says about them is more extended, more accurate, and far more useful, than what he says about diseases of the ovaries. He describes hydrosalpinx, pyosalpinx, and tubal pregnancy with perfect accuracy. He says: "It may happen that the fecundated egg may stop in the tube, and fix itself there and grow till it lacerate the tube and kills the mother." This sort of pregnancy in the Fallopian tube, he says, is not rare. He quotes De Graaf also, to the effect that the fringed edges of the tube may be found fixed to the ovary, producing sterility. He makes this most striking statement about the tubes—a statement which has lain dormant till its truth and importance have been sustained by my own clinical work and the pathological researches of Kingston Fowler, Lewers, and Griggs: "It happens, therefore, constantly, that there are found on the opening of dead bodies, illness and disorders (of the tubes) of which there are not the least suspicion. The ignorance of the patients concerning the greatest part of the diseases of the Fallopian tubes saves them the uneasiness they would have if they were better informed, and the physicians the necessity of judging of the consequences of these disorders."

Then comes a sentence, written in or about 1750. . . . "It is only known in theory that the inflammation commonly terminates by the way of resolution, which leads to a recovery; whereas those of abscess and gangrene are always fatal. The dropsy of the cavity of the tubes makes a slow progress, and the patients bear them a long time."

In regard to the prominent writers of the nineteenth century, we shall mention a few in whose works *Tubal Diseases* are considered.

The usually far-seeing Blundell knew little of tubal diseases, for he says, "the Fallopian tubes may be dropsical, scirrhous, affected by extra-uterine gestation, and so on, and they may be dismissed without further remark."

In 1832 Hooper published his work on the "Morbid Anatomy of the Human Uterus and its Appendages," and in this work he gave numerous illustrations of dilated and diseased Fallopian tubes. These illustrations Tait used in his "Diseases of Women," 1889.

In 1834 Heming published a translation of the " Practical Treatise on the Diseases of the Uterus," by Madame Boivin and Professor Dugès. In this work there is a chapter devoted to *Diseases of the Fallopian Tubes,* and in the Atlas which was published to accompany the work there are two plates which give excellent illustrations of dilated tubes.

In the " Treatise of Diseases of Females," by Dewees, published in 1854, he devotes a chapter to diseases of the tubes, and says " that these bodies are much more frequently inflamed than is generally imagined, we should infer from the number of instances of derangement of structure which dissection brings to light. Yet we are of opinion, that few practitioners have ever admitted to themselves that they were directing their remedial views to an inflammation of one or both of those bodies." Aran published his " Leçons cliniques sur les maladies de l'utérus et de ses annexes " in 1858. Pozzi says, " Aran, who was the first to perceive clearly the extreme importance of the part taken by the ovaries and tubes in uterine pathology, may be said to have anticipated his generation by attributing pelvic peritonitis to inflammation of the uterine appendages. He clearly showed that these were the foci around which gathered pus and false membrane."

In 1859 Arthur Farre contributed his well-known article to Todd's " Cyclopedia of Anatomy and Physiology," and in this article he gives an excellent account of the pathology of the tubes and illustrates this article with Hooper's figures.

In 1864 Churchill, in the fifth edition of his work on " Diseases of Women," devotes a chapter to Diseases of the Tubes.

In 1867 the New Sydenham Society published a translation of " Clinical Memoirs of the Diseases of Women," by Bernutz and Goupil, and there can be seen illustrations of distended pus tubes, and there can be found the following sentence : " Hence it is evident that a tumour having all the signs of a phlegmon may be similated by the existence of adhesions between the intra-pelvic organs ; and this more so, if there be any considerable purulent distensions of the Fallopian tubes."

We need not go on quoting any more works; we have given enough evidence to show that the ignorance displayed by Spencer Wells and others with regard to diseased tubes was very extraordinary because Wells had performed a thousand abdominal sections, and had made vaginal examinations without number, and yet he refused to believe in the existence of *diseased appendages*. Well might he, in after years, regret his words, "I have seen only one in my lifetime; perhaps they all go to Birmingham." [1]

[1] Tait, "Diseases of Women," 1889, p. 334.

XXII

Tait's Artery Forceps and the History of Hæmostasis

1883

We do not propose to give a full account of hæmostasis in surgical practice, but we shall touch on the important points in its history.

From the earliest time the importance of loss of blood was made manifest to the old-time warriors who saw their fellow-fighters die from hæmorrhage. The importance of hæmorrhage after wounds, whether inflicted in battle or in the course of everyday life, made this subject a vital one for surgeons in ancient times, consequently we find that ligature of blood-vessels was known in the days of Hippocrates, Celsus, Galen, and Paulus Ægineta, but they were not confident in its utility, and it remained for Ambrose Paré to show that the ligature was the proper means to apply in the practice of amputation. Before the days of Paré the surgeon removed a limb with reluctance, for he doubted his ability to arrest hæmorrhage, because they looked to the red-hot iron, or to the styptic, as their chief help, but Paré, having many times employed the ligature in recent wounds, determined to apply this means in case of amputation of a limb, and has earned a niche among the Immortals for his work.

Paré knew that one of the difficulties that he would have to encounter in the course of the amputation would be the seizing of the cut vessels, and so he had made for his use various types of forceps, which remained fixed on the vessel by the action of a spring; thus it was that forcipressure came into existence. Later on, promoting hæmostasis by pressure on the principal artery of the limb was evolved.

In the nineteenth century, when amputations became common in war and in civil practice for compound fracture, many different instruments were invented for grasping the principal vessels, and one is amazed at the plates in Seering's "Atlas," published in 1838, where are to be seen the innumerable varieties of hæmostatic forceps. Doyen [1] says:

> The promoters of the procedure of ligatures had in view the special object of avoiding the tortures of cauterisation and application of boiling oil, which had been almost exclusively employed in the time of Ambrose Paré. But the employment of "Patin's Valet" and other types of forceps, the furrowed jaws of which were infected with the most virulent forms of toxic agents, and of ligatures which acted as septic foreign bodies, produced disastrous results, and inoculated with an incurable septicæmia the unfortunates who had been temporarily saved from death by amputation. Thus it was that the cry of humanity which was raised by Ambrose Paré led to the sacrifice of myriads of human lives, through conferring on the victims the relatively small privilege of escaping the tortures of fire, after exposure to those of steel, and thereby removing the one great safeguard against infection.

But ligatures came to prevail by degrees, and still the instruments of hæmostasis multiplied, because there was a desire to do away with the ligature in many cases because of the inconveniences and the dangers which followed its use; the commonest danger being secondary hæmorrhage which occurred when the ligature separated; the clot being septic and not sufficiently adherent.

Then experience taught surgeons that the *mediate* ligature of arteries was a wrong practice, because they included in the ligatures tissues other than the vascular tissues. This led to the adoption of the *immediate* ligature, it being placed on the vessel when isolated from its cellular sheath. This proved more satisfactory, and as it was necessary to seize the vessel and pull it out of the tissue, instruments had to be invented for this purpose. Quite early Amussat and Maunoir recognised that the curling up of the middle and internal coat of the artery was the essential condition

[1] "Surgical Therapeutics and Operative Technique," English edition by H. Spencer-Browne, London, 1917, vol. i. p. 76. Translated.

for reliable hæmostasis, and they soon found that this was much more effectively realised by torsion than by the application of a simple immediate ligature. Consequently forceps were invented to seize the vessels, and hold them sufficiently firmly to enable torsion to be applied.

Doyen says that to Chassaignac belongs the honour of having first utilised *force* in the realisation of hæmostasis. Let us then trace the rise of hæmostasis by *forcipressure*. In 1824 Henneman de Schwerin published a memoir on the use of leeches, and was led to invent an instrument to arrest the bleeding in cases where the leech-bite bled to such a degree as to endanger the life of an anæmic patient. After this instrument he proceeded to invent the *pince de Henneman* which could be used during operations to catch the tissues and vessels, and this was the predecessor of the *pince de Carl Graefe*, introduced about 1851, and these were followed by the *serre-fine*, introduced by Vidal in 1848, and these in time were followed by the serre-fortes of Bedillot in 1865. The bull-dog forceps, so commonly used about this time by English surgeons, were a species of serre-forte, and Spencer Wells says: [1]

> I can hardly recollect when I first began to use forceps instead of the fingers of an assistant for temporarily stopping bleeding during operations; but I believe I learned it from Mr. Bowman before he left King's College. I have often admired, in the private and hospital practice of Sir William Fergusson, the ready way in which Mr. Henry Smith and the late Mr. Price would instantly stop a spouting vessel by the finger, and tie it, at a glance from the great operator. But I well remember seeing Mr. Bowman extirpate a very large tumour from the neck, and quietly put a "bull-dog" upon every considerable vessel as he divided it. This must have been in 1854, because when I went to the Crimea in 1855 I took a number of "bull-dogs" with me; and after my return in 1856, I never went to any serious operation without several different sizes; I used them all in my earlier cases of ovariotomy (beginning in 1858) for stopping any vessel which bled in the abdominal wall, divided in the first incision. I only allude to these now as a proof that the use of forceps as a means of permanently suppressing hæmorrhage has occupied the attention of English surgeons long before M. Koeberlé's inauguration in 1867.

[1] *Brit. Med. Jour.*, January 21, 1879.

We may note that Nunneley of Leeds, in 1867,[1] proposed to use a self-acting grip with crossed blades, which were attached to the bleeding points and left on for twenty-four hours. But it soon became obvious to surgeons that, while the serres-fines and the serres-fortes served the purpose of stopping hæmorrhage temporarily, instruments were wanted which would exert a much stronger force on the vessel-walls, and in order to satisfy this want the artery forceps came into being. If we go back to Paré we shall find that he had constructed, for ligation of vessels at the surface of amputation stumps, forceps of various forms, mostly adapted to fixed pressure. Doyen says that he seized with one of his forceps, or "crow's beaks"—genuine instruments of forcipressure—veins and arteries simultaneously with a little of the surrounding tissues, and tied all up together with a double thread. Dionis, in 1707, following the example of his illustrious countryman, says: "We grasp the extremity of the artery with a 'crow's beak,' or small forceps, furnished with a compressive ring known as the *valet à patin*; then sliding over the instrument and on to the artery a thread prepared and knotted, we tighten and secure it with a double knot."

The greater number of forceps employed by Paré and his followers, were adapted to continuous pressure, and they were kept closed by various forms of mechanism: rack, ring with catch, spring action, lateral or terminal screw bolts.

The *valet à patin*, or ancient pressure forceps, alluded to above, is described by Perret (1771) as being formed of two similar arms united by a hinge, and furnished with a spring which kept the jaws always closed; the opposed surfaces of the jaws were indented the teeth adjusting themselves to one another. This instrument served to hold one vessel while the ligature was being applied to another. This instrument, as pointed out by Doyen, is really a hæmostatic forceps constructed for continuous pressure.

To Heister must be given the credit of placing rings at the ends of the hæmostatic forceps, in imitation of scissors, and

[1] *Brit. Med. Jour.*, 1867, vol. xi.

of those forceps employed for dressings ; as a matter of fact the form of the well-known drainage forceps, used by Lister, is precisely that of Perret's dressing forceps ("L'Art du Coutelier," 1771).

For a period, however, the ring-handled forceps went out of favour, and the forceps with a sliding catch were used by many surgeons, and the "torsion forceps," as they were called, became the hæmostatic instrument favoured by Maissoneuve, and it remained in general use till the advent of Charrière's forceps. It was also used as a needle-holder, and the groves and depressions, found in the blades of these dressing forceps, were originally intended to adapt the blades to hare-lip pins or suture-needles. After a time the rachet was added and we got the original form of pressure-forceps, from which all other kinds have been modified.

In considering the pressure forceps, we must bear in mind the names of the various parts so as to be able to follow the alterations in the form of the forceps. Doran [1] says : "It may be said to consist of two 'blades,' bearing teeth and terminating in a 'nozzle' or 'points' ; of two 'shanks' continuous with the blades, connected by a 'pivot' or 'axis,' and made fast when closed by a 'rachet' or catch, bearing 'teeth' ; and of a pair of 'bows' or 'rings.' The 'rachet' or 'catch,' on the shanks of the forceps, placed just before the 'rings' were reached, was introduced by Charrière in 1855. It must, however, be remembered that the application of a rack-work locking apparatus to ring-handled and cross-action forceps dates back to a much earlier date, but we owe to Charrière the simple rachet that is invariably used now—that furnished with lateral teeth.

In 1865 Koeberlé asked Elser, a surgical instrument maker in Strasburg, to make for him a forceps somewhat analogous to Charrière's dressing forceps, provided with a rack fastening (goupille), which enabled the surgeon to grasp, and compress to some extent, tissues of various thickness. He just mentioned the use of the hæmostatic forceps in a paper published in Strasburg, in 1866. Revillout,[2] in 1868, made

[1] "Handbook of Gynæcological Operations," 1887, p. 94.
[2] *Gazette des Hôpitaux*, 1868, No. 75, p. 297.

known in Paris the new method of securing bleeding points which he had seen adopted by the Strasburg surgeon.

Péan had in his operations used the " serre-fines " of Sédillot, but in 1868 he got Guéride to make him some pressure forceps. Then began a controversy [1] between Péan and Koeberlé as to priority. Pozzi, in an interesting paper [2] on " The Evolution of Ovariotomy in France," published in 1909, alludes to this discussion and says :

> The main point in this discussion of priority, and one which does not seem easily to be decided, is whether Koeberlé's assertion is correct, that the forceps he was daily employing was adopted by Péan, more or less perfected, after the publication of Revillout, or whether Péan's is the correct assertion, viz. that he was the inventor of the instrument when operating at Paris under the same necessity as Koeberlé at Strasburg ? All the same, whether he invented it first or at the same time, Péan, owing to his high position in the Paris hospitals and to his large practice, helped most considerably to make the use of the hæmostatic forceps general, and his name with that of Koeberlé must remain attached to the method.

Contrasting Péan's instrument with that of Koeberlé, we find that Péan's forceps were very light and the grip of the blades was very weak, and it had an advantage of Koeberlé's instrument because the shanks could be unfastened at the pivot so that the instrument could be cleaned. But both these forceps had a disadvantage, for a space was left between the shanks when they were closed, and this space admitted the entanglement of one instrument with another, or of the passage of omentum or other structures. Spencer Wells saw these mistakes, yet when he had his first instrument constructed, it also had this defect. It also had only a single tooth at the rachet, known in these days as *Matthieu's catch*. In his later form he had his instrument made lighter and shorter, but still it was much heavier than Koeberlé's instrument. The shanks were so constructed that the upper entirely and accurately covered the lower when the catch or rachet was closed at the second tooth, and so there was no space between the shanks. The instrument was really

[1] This is set out in " Du Pincement des Vaisseaux," Paris, 1877.
[2] " Surgery, Gynæcology and Obstetrics," October, 1909, p. 420.

brought to a high degree of perfection through the care bestowed upon it by Hawksley.

This was the instrument that Tait modified, and then recommended in the fourth edition of his "Pathology and Treatment of Diseases of the Ovaries" (1883). The characteristics of Tait's forceps are that the outer surface of each blade is exceedingly convex, the curve passing on to the shank above the level of the pivot. Then, again, the point is not nearly so blunt as in Wells' instrument, and Tait's idea in making it this way was to enable the loop of the ligature to slip down towards the point and make the application of the ligature much easier; Greig Smith later on modified Tait's forceps and made the teeth run parallel to the outside of the blades and not transversely. He thought they had more holding power than forceps with transverse teeth; the tissues are caught and held sharply compressed along two lines, and are flattened out, nor can the vessel be squeezed out of the grasp of the blades.

In later years, the man who contributed more than any other to bringing forceps to great perfection was Doyen, and to him we owe the forceps with multiplying powers, by which it is possible to reduce the interposed tissues by a very great pressure, so that they are crushed between the jaws of the instrument and are agglutinated in a few minutes, under a pressure of 500 to 2000 kilogrammes, to a structure of the thickness of $\frac{1}{4}$ to $\frac{1}{2}$ millimetre. This instrument Doyen calls his *écraseur*.

XXIII

Trip to Canada and America

1884

Tait had found time in 1883, as we have seen, to expand his Hastings Prize Essay into a treatise on "Diseases of the Ovaries," and his operations had become so numerous that in 1884 he published his first recorded 1000 cases of abdominal section. His fame was extending on all sides and to Birmingham came numerous American surgeons who wanted to see his work. One of these, Dr. Van der Veer, persuaded Tait to take a trip to America and demonstrate his operations in some of the big towns. He decided to do so, and set out for Boston in 1884. On his return he contributed the following notes to the *Birmingham Medical Review*.

AMERICAN NOTES [1]

A trip across the Atlantic has now been made so slight an undertaking that it certainly cannot be the subject of a paper such as might have been written some twenty or twenty-three years ago, yet it seems that so few of our medical brethren on this side of the world take advantage of the facilities for the journey, that it is quite possible I may be able to say something of interest to you about it. This is the more likely to be the case as I undoubtedly was favoured with most exceptional opportunities of meeting my professional brethren during my brief stay in America. I saw them under all kinds of circumstances, in their hospitals, in their classrooms, in their private practice, and in their domestic circles, to the fullest advantage. They come over here in numbers, so large that I had gained an impression that they must be a much richer body than we are, for it certainly is a very unusual thing in this country to find a practitioner, however eminent, taking a voluntary holiday for three or four months, or even so long as a year, yet we find this is quite a common luxury amongst our transatlantic brethren. I felt a considerable amount of curiosity upon this subject, and therefore

[1] *Birmingham Medical Review*, December, 1884.

I made inquiries, the result of which quite convinced me that this practice has not its origin in greater wealth or more freedom from competition; on the contrary, I found that it springs from an earnest and laudable desire to extend their knowledge and increase their powers of usefulness, that they may thereby stand higher in the estimation of their clients. They have in America institutions which are quite unknown in Europe, called post-graduate colleges, and in this they seem to me to recognise to a much greater extent than we do, that after graduation a doctor's medical education is just about to commence. Thus in New York there is a large institution called the "New York Policlinic," a school of clinical medicine and surgery for practitioners. It is described as "an institution in which there are didactic lectures given," practitioners only being admitted, and the class being limited so that each case may be minutely and leisurely examined and thoroughly understood. The list of teachers includes some of the most eminent men in the various departments of practice in New York. It is managed by the Board of Directors, which includes Fordyce Barker, Hastings Hamilton, Addis Emmet, Lomas, and Weber.

I am certain that there is nothing of this kind in Great Britain, and I never saw anything on the Continent with which I could compare it. American doctors regard a run over to Europe every few years as a kind of class-taking, in a cosmopolitan post-graduate college, and from what I have seen of surgical practice on our own continent, in this country and in America, I have no hesitation whatever in recording my opinion that it would be vastly beneficial to both continental and English surgery if return visits were made in larger numbers. I do not say we are lacking either in surgical culture or in earnestness of purpose, but on the Great Western Continent there is an enthusiasm, a vivid interest in life, a regular go-a-headness of which it would be well if we had a stronger infusion. The general summary of my visit to the United States, and to Canada, may be briefly put in the statement that no Englishman can obtain a reasonably full grasp of how the world is moving, or of the numerous phases of life, medical and surgical, as well as others, until he has seen life across the Atlantic.

I purpose in this paper only to tell you in a running sketch what I saw from day to day as I passed from one city to another, and to give you my general impressions of what I saw. This will answer the request made to me far better than any elaborate disquisition upon specific subjects, as such a paper would require a much longer stay, and a deeper critical investigation than was possible in the time at my disposal. Let me say here in passing just a word upon the subject

which was raised by a paragraph in a recent number of one of our leading medical journals. I mean the position and competency of the surgeons on the "Atlantic Line steamers." Of course I only saw two of these gentlemen, one on my outward journey, and the other on my return. But if I may take them as samples of their class, I do not hesitate to say that the insinuations contained in the paragraph in question amounted to a grave mis-statement. It so happened that on both of my journeys, there were cases on board of very considerable gravity, which as a matter of course formed a subject of professional conversation between myself and the surgeons of the ships, and I express my opinion without the slightest hesitation, that for culture and professional competency these two gentlemen were quite equal to anything which is usually met with in our life on land. As regards their social position, they are treated thoroughly well by the companies in whose service they are, and by the passengers with whom they are in daily contact; and I would point out that no kind of life can be regarded as more harassing than that in which a man may be shut up for ten days with a case of the gravest possible kind, and absolutely removed from any advice or assistance from professional brothers, save by the accident of some of the passengers belonging to the same craft.

I never fully understood what a terrible thing this isolation may be, until I had a little experience of it, on my own account, on board one of the steamers on the St. Lawrence. I left Montreal about eight o'clock in the evening for the night journey to Quebec. About an hour after I was comfortably in bed and asleep, I was roused by one of the officers of the ship, to whom my presence on board had been communicated, to come to the assistance of a young lady who was said to be bleeding to death. I hurriedly dressed, and when I reached the ladies' cabin I found that the sufferer had accidentally broken a pane of glass with her elbow, and had inflicted a severe wound on the fleshy part of the arm, from which she had lost a very large quantity of blood. The hæmorrhage had fortunately been arrested by a rude kind of tourniquet, and round the couch were gathered a number of terrified priests and nuns who were engaged in arranging a service for the dying. A steward held a smoky oil lamp, and the helpless captain stood by with an antiquated medicine chest. It became at once perfectly clear to me that the brachial artery had been injured, and so much blood had been effused amongst the tissues that the task of securing the bleeding points would be no easy one. The only instruments available were a bleeding lancet, a pair of dissecting forceps, and an old-fashioned aneurism needle. Fortunately amongst

the drugs there was a small bottle of chloroform, and after much difficulty in overcoming the terrors of those who surrounded me, I put the patient under chloroform, and an incision over the place where, after a lapse of some twenty years, I still had an idea the brachial artery might be found. Luckily for myself, still more luckily for the child, I stumbled upon the vessel and tied it, and I venture to say, I never in my life was more devoutly thankful than to see that patient, three days afterwards, in a condition of convalescence. I have since been informed that her recovery is complete.

After I landed, the first city I visited was Boston, perhaps the most famous of the cities of the States in medical as in other matters. Amongst the members of its medical profession were many old friends, and during the few days I stayed there I think I may say I was enabled to see everything. In the first place, I saw a monument erected not so much to an individual as to the accomplishment of a great discovery, the establishment of a great principle. In the large square of the city a graceful erection of stone, known as the " Ether Monument," commemorates the date upon which it was ascertained that a vast amount of human misery could be relieved with mathematical certainty, and the date from which the greatest triumphs of surgery must be reckoned. I need not tell you that to myself this monument had a special interest for without anæsthesia the whole realm of work to which I am devoted could never have been built up. Thence I was taken to the Massachusetts General Hospital, where I saw the sponge upon which the ether was given to the first patient upon whom an operation was performed, while in a condition of profound artificial sleep, and the theatre in which this great triumph was achieved remains much the same as it was then. The hospital itself is a magnificent building of granite, built very much on lines of hospitals of its time. It was built in 1784, and on account of its immense solidity, the original building seems to have been very little altered, though many additions have been made to it. One of these is of a very noteworthy character in the form of a new out-patient department, recently erected at the cost of a young gentleman now studying medicine within its walls. Originally built outside the city, like our own institution in Summer Lane, the hospital is now surrounded, though not closely, by buildings on all sides, and the rapid growth of the population here as elsewhere in America may be well indicated by the statement made to me by one of my surgical friends, Dr. Homans, who is attached to the hospital. He is by no means an elderly man, yet he could tell me that whilst a youth he had often shot wild ducks in the hospital grounds.

It was, of course, towards the practice of abdominal surgery

that I directed most of my inquiries, and I found that in Boston its progress is rapidly becoming satisfactory. Dr. Homans visited me here about two years ago, and he has confessed himself as having been largely converted to my views and methods of practice. He showed me a series of fifteen cases, some of them of an extremely serious character, all in progress of recovery, and this in spite of the enthusiastic Listerism to which he subjects all his patients. But his methods have become very much simplified by his experience in England, and he was good enough to admit his indebtedness to what he saw here. He does not perform any of these operations in the public hospital; most of them are accomplished in a small and very well regulated institution at the head of which is an English lady belonging to a sisterhood. During the last twelve months he had performed twenty-seven ovariotomies without a death.

Perhaps the most interesting building in Boston to me was the new medical school, which forms a department of Harvard University. This building was opened on the centenary of the establishment of the School with an address by Dr. Oliver Wendell Holmes, the Emeritus Professor of Anatomy, and in the theatre devoted to the teaching of that science I saw a most striking portrait of the "Autocrat of the Breakfast Table," appropriately placed on the scene of his labours. He has secured himself a niche high in the temple of fame, as a poet and as a writer of fiction, but he has a still higher place in the memories and the affections of the thousands he has taught in this school. Dr. Holmes delivered an address of marvellous eloquence at the opening of the new building of the Medical School on the hundredth anniversary of its existence, and some of the passages in it are worth quoting, alike for the brilliancy of their language and the quaintness of their thought. He tells us, for instance, that "we are in the habit of counting a generation as complete in thirty years, but two lives cover the whole century by an easy act of memory. I, who am now addressing you, distinctly remember the Boston practitioners who walked among the dead after the battle of Bunker's Hill, and pointed out the body of Joseph Warren amongst the heaps of slain." He tells his hearers that then there were "Three teachers only, where we have forty or nearly that number! But when the great University of Gottingen was established the illustrious Haller filled the one chair of Botany, Anatomy, Surgery, and Medicine. I called it a chair. It was rather a settee of Professorships."

The buildings themselves are certainly the most perfect in their arrangement, and the most complete in their appointments, of any buildings I have ever seen erected for a similar purpose with perhaps the exception of the new buildings of

the University of Edinburgh. And finally of this beautiful building, the autocrat of the breakfast table thus speaks at the close of his address. "It stands solid and four-square among the structures which are the pride of our New England Venice—our beautiful metropolis, won by well directed toil from the marshes, the creeks and the lagoons, which were our inheritance from nature. The magnificent churches round it let in the sunshine through windows stained with the pictured legends of antiquity. The student of nature is content with the white rays that show her just as she is; and if ever a building was full of light—light from the north and the south—light from the east and the west—light from above, which the great concave mirror of sky pours down into it—this is such an edifice. And over all this rise the tall landmarks, which tell the dwellers in our streets, and the traveller as he approaches, that in the home of Science, Arts, and Letters, the God of our Fathers is never forgotten, but that high above these shrines of earthly knowledge and beauty are lifted the towers and spires which are the symbols of all human aspirations, ever looking upward to Him, the Eternal, Immortal, Invisible."

On the library table, bearing marks that it was not merely "a presentation," but a well-used copy, was Mr. Sampson Gamgee's book on Fractures—a fragment of comfort for my provincial soul. In the Warren Museum I saw many preparations, put up by the hand of Warren himself: a hand, the bony elements of which I had the privilege of touching; for with a quaint conceit, entirely characteristic of the man, he now himself forms part of his own museum. I need not remind you that the book which gave to the world the results of Warren's labours on the pathology of tumours was in some respects an epoch-making contribution to surgical pathology. Of John Collins Warren, Oliver Wendell Holmes tells us that he was "a cool and skilful operator of stern ambition, equipped with a fine library, but remarkable quite as much for his knowledge of the world as for erudition, and keeping a steady eye on professional and social distinction which he attained and transmitted."

The main building of Harvard University is situated in the town of Cambridge, about four miles from the medical school, and it has been so often and so perfectly described that I need not say anything in repetition, save to speak of the new Memorial Hall, erected as a Valhalla for the alumni who died in defence of the unity of their country. I doubt if throughout the world there is to be found a nobler or more touching, or probably more enduring, memorial of heroism than the beautiful vestibule of this building, where, upon plain marble slabs are recorded the names and places of

death of the students of Arts, Law, Medicine, and Theology, who died in their country's service during the terrible years between 1861 and 1865.

By no means the least striking feature of American medical life is its abundant hospitality, a feature which runs through life generally in the United States. We have here our little medical social clubs, which have the useful functions of promoting good fellowship and a wider culture of professional interests, and very many of us belong to these. But I have never seen this feature of our professional life carried out to the same extent as it is to be seen across the Atlantic. It happened that the night on which I arrived in Boston was the time of one of the monthly meetings of such a club, and I was carried off to meet some fifteen or twenty men, the names of most of whom are well known on this side. There were physicians and surgeons and specialists and general practitioners full of learning and of the quaintest fun. We started on a small steamer from the village of Waltham, where is situated the enormous factory for watches, and after some ten or twelve miles run up the river, the beauty of which certainly equalled that of anything of the kind I have ever seen, we were landed at the country house of Dr. Chaunce Blake, where we were entertained right royally. While there I witnessed one of the most effective sights I ever saw brought about by a fierce storm of lightning playing through the trees of the forest in which Dr. Blake's house is situated.

From Boston I passed on to Montreal, where began the series of engagements for the fulfilment of which my trans-atlantic visit had been arranged. The beauty of this city has been so often praised that it is useless for me to repeat the platitudes of my own impressions further than this, that in my memory there dwell above all others in prominence the recollections of three landscape views that I have seen. From the terrace at Malvern, from Arthur's seat at Edinburgh, from the Castle at Heidelberg, and last but not least from the hill at Montreal, from which extends a view all round and not surpassed by anything I ever saw. In Montreal I was the guest of Dr. Gardner, Professor of Gynæcology in McGill University, and it was my privilege to give an address to the Canada Medical Association, the annual meeting of which had been arranged to take place during the three days just preceding the meeting of the British Association. My business here, of course, is chiefly with what I saw and not with what I said, but I wish again here in my own land to repeat my acknowledgments for the brilliant reception given to me by my Canadian brethren, not so much on my own account as for the position in which they placed me, that of a representative for the time being of British surgery. During

those three days I was associated with some 300 practitioners of medicine, I heard a number of papers read with discussions upon them, and I say without hesitation that nothing which was said or done at that meeting, but would have reflected credit on any medical gathering in the world. I often hear it said by practitioners in this country, whose lots are cast in places remote from the busy centres of life, that they find it difficult or even impossible to attend meetings of professional societies, and to keep themselves abreast with the growth of the science of medicine and surgery ; but in that new country, and at that Congress, I found men eager and able to be present though they had thousands instead of scores of miles to travel, and it was to me quite impossible to realise the fact that men who sat next me, and who talked fluently and well of the most recent advances in pathology, who knew all the dodges and newest things in laryngology, otology, gynæcology, practised in villages, four, five, and even six days' travel from the place of meeting, that many of them existed in places still unmarked on the map, without any professional neighbour nearer than perhaps a hundred miles. Some of them were even professors in flourishing medical colleges placed in large cities, which ten or twelve years ago had no existence. In the style, character, and conversation of these men, not only could nothing be detected which could mark them as being defective in general or professional culture and education, or which could place them in a rank lower than the practitioners of my own country ; but I doubt very much if, from the highest to the lowest in our own ranks, we were to take 250 or 300 of our men at random, we could compare favourably with them.

Instances occurred every now and then to me of a most delightful kind in coming across suddenly and unexpectedly faces familiar in college life many years ago ; faces of which I had lost all recollection, and of the history of whose owners I had no knowledge at all. Men who, tired of the struggles of medical life in the old country, had settled in the new world, and had become prosperous, happy, and successful. One unfailing source of wonderment, which no amount of explanation has as yet made clear to me, is the much larger proportion of practitioners to the population which exists on the American continent compared to what we have here. In England we have about one doctor to 1400 people. In Canada it seems to be about one to 800, and in the States it seems to be about one in 600 or 700 ; yet they seem to be better paid, to be less hardly worked, to be more prosperous and successful than we are here, and to be in much better social position than we can boast of. The latter fact most especially struck me, and it was proved to me in a great variety of ways. But

perhaps I cannot give this impression more clearly, than by taking an extract from the speech of a Boston physician recorded at the time of the opening of the New Medical School. He gives his impression as it were from the other side, and certainly there is a singular concurrence in our experiences. "Dining," he says, "with two Englishmen a few years since, one an Oxford Professor, and the other the brother of a lord, I was surprised to hear their views on the social standing of the medical profession, and could not help contrasting their position here, where, if not all autocrats, they are all constitutional, and some of them hereditary, monarchs accompanied by honour, love, obedience, troops of friends."

Another source of surprise was the large number of medical schools; thus in Montreal, a city of 141,000 inhabitants, there are no less than four of these schools—two Catholic and two Protestant—and although there is only one of great importance, still all of them are well officered and well appointed, and from what I could see of the results of their training, I am unable to say that any of them can be charged with inefficiency. In Toronto there is a magnificent University, the President of which is the famous archæologist, Daniel Wilson, and two medical schools, the buildings of both of which I inspected with care, and I venture to say that they compared very favourably with the school in our own town, or indeed with any provincial medical school as well as with a large number of our metropolitan schools. The tendency towards the downward competition, which would otherwise be inevitable in medical education as a result of an undue number of centres of medical education, is prevented by the establishment of that which we most of all want in this country, a guarantee on the part of the State of a minimum amount of medical education. The only defect of this State control, so far as I could understand it in Canada, is that all the provinces are not agreed as to what this minimum shall be, yet each province seems quite capable of protecting its own interests. At one meeting I heard a very remarkable and exhaustive speech on the subject made by my friend Dr. Howard, the Dean of the Medical Faculty at McGill College, and I was very much pained to learn from him that they have to contend in Canada against exactly the same evil influence which we object to here, exercised by a few of the British medical corporations. With so many degree-granting bodies as they have, it is perfectly impossible to expect a uniform quality of education. It happens of course that some of the colleges pass inferior men who are rejected by the State Examining Board. These rejected ones are very much in the habit of running over to Great Britain and speedily returning to their native province

armed with a parchment granted by one or other of three well-known corporations in this country, all of whom shall for the present be nameless. As British qualifications these diplomas, I understand, are allowed to override the decision of the State Examining Board, and thus mischief, which we might well confine to our own borders, is carried across the Atlantic. The complaints on this subject are loud and frequent, and they add another to the many strong arguments which we are at present urging in favour of the reform of medical education. Only one other fact concerning this matter needs to be alluded to, and that is that these State examining boards, which are practically the general medical councils of the province, though technically appointed by the Government, are subject to the universal suffrage of the profession.

Of the hospitals of Canada, I can say nothing but what is favourable. Dr. Hinkston, the distinguished surgeon of the Hôtel Dieu and others, took great trouble to show me all their details. Their appointments are equal in every respect, and in some respects are far superior, to those to be seen in any but the newest hospitals in this country. I spent a long afternoon in the hospital at Toronto, and I saw there the results of surgical work as brilliant as any to be found in Great Britain.

Unfortunately the time of my visit to Canada was such that the schools had not re-opened for the winter session, so that I saw nothing of them in actual work.

Of the cities of the States, the first I visited was Albany, the capital of the State of New York, a city famous as being the seat of some of the earliest Dutch settlements. The cities of the United States differ in many marked respects from those of Canada, and many differences quite as striking are to be found in the characters and habits of their peoples, but these differences do not extend into matters affecting my medical notes. In Albany, I found two large hospitals, one of Catholic foundation, and the other a public institution somewhat under State control. I was the guest of Dr. Van der Veer, who is attached to both of these institutions, and he was kind enough to give me opportunities of meeting the staff of both of them, and what must be considered by far the greater number of the profession of the city and its neighbourhood. Here, as in Canada, I was very favourably impressed with the style of the men I met, and here again, was pained by the fact I have already alluded to, evident on all sides, that my profession in the States has a social position and influence far greater than it enjoys here. There is in Albany a very large and successful medical school with about 250 students. There is an abundance of clinical material,

the students are a very earnest and well-conducted body of young men, and their professors uniformly give them a character for steadiness and decorum, which perhaps might excite the envy of some similar bodies here. I had an opportunity of delivering two clinical lectures at this school, and I was asked to perform operations in each of the hospitals, both of which tasks I undertook with very considerable hesitation. In the first place, as I appeared before my medical brethren as a representative of British surgery, and as I never in my life had any opportunity of delivering such a thing as a clinical lecture, I very much feared my performance would prove extremely unsatisfactory; for the delivery of clinical lectures, like anything else, can only be well done by those who are accustomed to it. Our arrangements in England, in this particular, certainly do seem to be defective; for I could hardly get my American hosts to believe it possible that any one who has at his command such a mass of material as passes through my hands should never have had one single opportunity of making that material the means of instruction.

The operations I undertook with still greater hesitation; for I had so often and so strongly objected to the performance of abdominal operations in large general hospitals, that I could not undertake their performance without laying myself open to grave charges of inconsistency on the one hand, or, on the other, of subjecting my patients to an unjustifiable risk. It was pointed out to me, however, that I had been invited to America as a sort of missionary, and that in so far as the kind of work I did and its results had been made the subject of misrepresentations, and mis-statements, it was my duty to emphasise my missionary statements by actual performances, and that this could be done most completely before large audiences of practitioners and students. Further, they argued that the mere performance on this ground of such an operation under the circumstances to which I objected would counter-balance, and even justify, the increased risk to which I believed the patients would be subjected. Upon this specious reasoning I was persuaded to perform a number of operations, with the result that all of them recovered. I am therefore somewhat shaken in my belief that the risks are so great as I had asserted them to be; but, on the other hand, as they were all done without the slightest antiseptic precaution, I am more than ever persuaded that ill results are to be accredited rather to bad surgery than to malevolent germs. One of the cases at Albany was singularly satisfactory, as it included an accurate diagnosis of a solid ovarian tumour, and of uterine myoma, and ended in the removal of both. In New York, at the Bellevue Hospital, I had an equally

satisfactory effort in being able to diagnose and successfully deal with a very typical instance of pyosalpinx.

I need not say that the subject of "Listerism" was one upon which I evinced much interest and curiosity, and made many inquiries about it amongst my professional brethren in America. Perhaps I may also assume that what I have to say about it will be taken by my hearers with the qualification that as I have now, for over three years, ceased either to accept Lister's doctrines or to follow his practice, and have obtained increasing success thereby, I may be regarded as more likely to collect evidence in favour of my own views than against them.

The statements made and the rules laid down by Lister and his disciples have been always so precise, not to say dogmatic, that one of the great difficulties which I always have had in connection with the scheme, was that the practice and details recommended were subject to such constant alteration, that from month to month there had to be a constant replacement of old plans by new ones, and there was a constant and endless variation in the details of the treatment. This has always seemed to me to be absolutely fatal to any logical discussion of the doctrines upon which the practice is based. If we assume, what nobody can doubt, that specific diseases are produced as the result of the growth within the body of specific entities, and if we can have an assured statement that by chemical or mechanical treatment these living germs, or whatever may be the last word which has been used to identify them, can be destroyed, the whole question of such diseases is brought within the limits of mathematical certainty.

Prevent the access of the disease-bearing entity, and the disease will not occur; nothing can be simpler. But the followers of Lister proceed a little further; they assume that cases of death after operations have specific characters, as clear and distinct as those, for instance, of scarlet fever and small-pox. And from this assumed analogy, and upon this assurance, they base and establish all the infinite and increasing variety of processes for the destruction of the wicked germs. But from my point of view I fail to see the analogy which is assumed. I have for years been struck with the fact that the conditions under which patients die after operations, assume such endlessly varying forms, that I have come to the conclusion that any doctrine which takes for granted that these conditions can be so grouped as to build up forms of specific disease, is based entirely upon insufficient observation and inaccurate knowledge of the facts. This view has obtained a stronger and stronger hold upon me, as my experience in my special line of practice has accumulated. Watching

cases from time to time that had done badly, I am satisfied that hardly two of them died in precisely the same way and from precisely the same cause, and I am therefore profoundly convinced that to assume that every patient, or at least the majority of the patients, who die after abdominal section, succumb to some septic peritonitis or some septic something or other, is only to cover our ignorance with a cloak, and to bar the way for real advance.

I do not think that any contribution to the literature of abdominal surgery has done so much harm as that column in Sir Spencer Well's book, in which a terribly long series of fatalities is ascribed to "septicæmia," and "septic peritonitis." Some of Sir Spencer's deaths are put down as having occurred from "peritonitis" simply; but throughout the book, from board to board, there is not a word which explains the different use of these three terms. I should like immensely to be told how a pathologist is to discriminate between peritonitis that is septic, and peritonitis that is not, and it is of the utmost importance that such a difference should be clearly defined, for it is the merest folly to put up a theory and then distribute manufactured instances around for its sustenance, as has been done by Sir Spencer Wells.

I do not mean to say that there may not be specific germs which may be conveyed into the abdomen of the patient by accident, germs which may kill the patient, but I do say that the evil influence of Lister's teaching has been over and over again instanced in the constant tendency shown by too many of his disciples trying to cover faults that could be avoided, and bad surgery that might be improved, by attributing the disaster arising from them to a supposititious germ. The conclusion of this part of what I want to say is this—that after innumerable statements that this plan, that detail, or the other substance, an all-permeating spray, a piece of lac plaister or a mass of thymol gauze, or eucalyptol, form irresistibly destructive agents for the germ, we have the astounding fact that one after another these nostrums are given up, declared to be inefficient, something new is advanced, in its turn to be discovered as a fraud, and thus year after year the process is repeated. (It is somewhat curious that since this was written, I have read the address of Sir Joseph Lister at the Clinical Society, in which he admits having twelve months ago expressed an overweening confidence in his previous agents, and now he recommends an ingeniously concocted mixture of blood serum and corrosive sublimate with a confidence as great, and probably as overweening, as in previous instances. This constant and confident advance and humiliating retreat is quite enough, if no other argument were needed to show the fallacy of the whole thing.)

These were the views which in very many instances I laid before American surgeons, and I did not find in a single instance that the facts could be disputed. Amongst all with whom I talked I found only one who still used what I suppose I may, without any disrespect, call the old-fashioned carbolic method, with the spray and protective and solution, and plaister and gauze ; so far as I could discover the days of that sort of thing on the American continent are rapidly coming to an end.

Our transatlantic cousins are greatly infected by German methods of proceeding, and I understand that the last new germicide is corrosive sublimate. A large number of the American surgeons now perform their operations under an irrigating stream of a very weak solution of this salt, but they are by no means agreed either as to the strength of the solution required, or of the completeness with which it should be used. One of the most distinguished of American surgeons told me that he is now quite satisfied that sponging over the wound, after the operation is completed, with a 0·1 per cent. solution is all that is necessary, and he states that his results are quite as satisfactory in every way as when he used the cumbrous, expensive, and troublesome method involved in the use of carbolic acid.

The next city that I visited was one famous in the annals of all historical affairs in the new country. I mean Philadelphia ; and no place in the United States can give a better idea of the enormous progress which has been made in America, in every phase of life. When I reached that city the medical schools had opened, and I was able to see them at work. I was invited to lecture and demonstrate in the Jefferson Medical College, and was honoured by a very large audience of practitioners and students. Here as elsewhere I can say nothing but in praise of the style and appointments of both schools and hospitals ; perhaps of all the schools which I visited the one which struck me most, is the Women's Medical College in Pennsylvania, which issued in May last its thirty-fifth annual announcement. It has a body of teachers twenty-three in number, of whom a few belong to our own sex ; but it is worth while saying that the demonstrators of anatomy, surgery, practice of medicine and obstetrics, are all ladies, and the Professor of Obstetrics is Dr. Anna E. Broomhall, who favoured me with a visit here last year. The building in which the school is conducted is very large and splendidly appointed, and attached to it there is a large General Hospital with lying-in wards. The staff of this hospital is entirely feminine, and in its theatre all the operations of surgery have been successfully performed by lady surgeons. Last year twenty-six degrees of Doctor of Medicine were granted by

the faculty, and from the perusal of the curriculum, as well as conversation with some of the graduates, and by discussion of the merits and demerits of the whole question with some of its friends and some of its opponents, I am quite satisfied that the lady graduates of Philadelphia are quite as carefully trained as those in any other medical school. When I tell you that last winter one hundred and thirty-two students matriculated in this school, that the theatre in the hospital is large enough to hold three hundred sitters, and that about four thousand patients every year pass through the hospital, I think I shall have said enough to prove to you that, in the United States at least, the practice of medicine by women has become quite an accomplished fact. There can be no doubt from what I heard from their male competitors that large numbers of these women succeed in securing a fair share of public confidence. Unfortunately, although they have received a much fairer share of assistance from their medical brethren in America than their sisters have here, their path has not been quite free from obstruction, and in many parts of the States they are not yet admitted to the equality of professional position, which I have always claimed as their right. Yet I must say on behalf of the medical men in America, that they have behaved far more generously to women desiring to study and practise medicine than we have in this country. On both sides of the Atlantic the prejudice against them, altogether unnecessary and entirely without justification, is slowly passing away, and I hope ere long to see measured out for them everywhere that justice which their claims entitle them to.

The views which I personally expressed in a paper read before my professional brethren in this town twelve years ago, have received ample justification from my experience in Philadelphia. These views at the time brought down upon me words of the strongest condemnation ; I might repeat them here, but I prefer to take them as expressed almost completely by Dr. Oliver Wendell Holmes in the address I have already quoted, " I have always felt that nursing was rather the vocation of women, than general medical, and especially surgical, practice. Yet I myself followed a course of lectures given by the younger Madame Lachapelle in Paris, and if here and there an intrepid woman insists on taking by storm the fortress of medical education, I would have the gate flung open to her as if it were that of the city of Orleans, and she were the Joan of Arc returning from the field of victory."

I do not think that any other point to which I might call attention would justify me in repeating an account of details which would present but slight variation, and the only other incident of which I may speak was my visit to Washington,

where I had the privilege, with the help of the Director General of the United States Army Medical Service, of seeing one of the most wonderful medical institutions in the world. Of course I allude to "that immense library formed, but always forming, at Washington, and one of the results of which comes before us in that colossal catalogue which is one of the best proofs of the advancing civilisation of the Great Republic." You are all familiar enough with the Index Medicus, the work of Dr. Billings and his assistants, and any one familiar with the business of cataloguing must know what labour that represents, but if you could see the library itself your impressions of the undertaking would be immensely increased. The building in which it is situated is the old theatre hallowed by the occurrence in it of one of the most dramatic incidents of modern times, I mean the assassination of Abraham Lincoln. The building also contains a large museum devoted mainly to the exhibition of specimens of surgery, everything from the use of all kinds of warlike weapons, and there are exhibited the ghastly results of Lincoln's war, every conceivable injury inflicted by lethal methods is collected, gun-shot wounds of bone form one of the most extraordinary sights I ever saw in my life, and some of the specimens have an attraction not merely of a surgical character. Thus I saw the cervical vertebræ of Lincoln's assassin, split by the bullet which reached him in his hiding place, and the pathological collection aptly includes the enlarged spleen of Charles Giteau who murdered President Garfield.

My trip ended in New York, where I met a reception from the leading members of the profession, and from many distinguished laymen, of the heartiest and most friendly kind. I lectured and operated at Bellevue Hospital before a very large audience, and I was taken round several of the other medical institutions of the city. Here, as everywhere else, I was immensely impressed with the hospitable friendliness of the people, and in conclusion I can, I think, say that if any one here requires to be persuaded that the Anglo-Saxon race and the Anglo-Saxon tongue are destined to be for the future, as they have been in the past, the dominant influence of the world, he must do as I have done, and put it within his power to write a similar rambling tissue of American Notes.

XXIV

Tait's Influence on the Development of Pelvic Surgery in America

1885

Tait's visit to America in 1884 exerted a great influence on the development of pelvic surgery in that country.

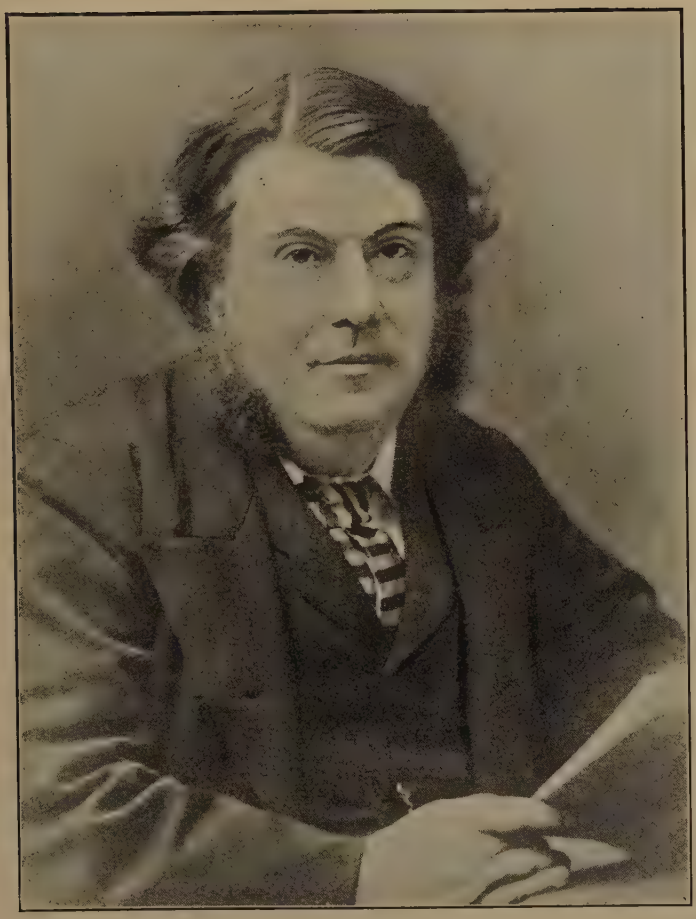
From time to time American surgeons had visited Birmingham, and had watched Tait's work and had noted his simple aseptic technique, and on returning home they had put into practice what they had learned, and finding that his teaching was correct, he, as the years passed, had a large and enthusiastic following in many of the clinics in the large centres in the United States.[1]

In 1883 Tait was able to write:

> The class of cases in which relief is afforded to patients by the removal of an occluded and distended Fallopian tube, associated perhaps with cystic disease or chronic inflammation of the ovaries, has, as yet, received no recognition in this country outside Birmingham. There must be hundreds of women suffering in this way in London, but I have as yet heard of no operation of the kind having been performed there. A large number of my preparations are in the College of Surgeons' Museum, and I have read papers on the subject at the London societies; but the first recognition of my work has come from America. In September last, Dr. Emmett of New York paid me a visit and saw some of my cases, and on his reporting what he saw, my example at once had an influence. He took away with him some of my specimens, and on December 21, Dr. T. Gaillard Thomas reported to the New York Academy of Medicine four cases of the kind I am now discussing—tubular cysts with diseased ovaries. The symptoms in his cases are precisely those I have described,

[1] "The incredulity which was so freely expressed in this country by people who would not come to see for themselves, was speedily overcome by the evidence of those who came in large numbers from America."—*Brit. Med. Jour.*, May 19, 1886.

PLATE XXIV.



LAWSON TAIT IN 1885.

To face page 252.

such as recurrent pelvic peritonitis, intense pelvic pain, and profuse menorrhagia, no benefit from treatment, and increase of the suffering from the use of pessaries. The condition of the uterine appendages he describes just as I have described them: "ovaries covered with small cysts, and the tubes enormously distended with fluid, giving them the appearance of sausages." With equal correctness he describes the difficulties of the operation, and he gives to me an amount of praise and credit, with which it would be false modesty to say I am not deeply gratified. The complete confirmation of my work by such an authority as Dr. Gaillard Thomas is all I can desire.

One of his most enthusiastic followers was Joseph Price, who had paid Tait a visit at Birmingham, and gave him in return warm welcome when he visited Philadelphia. Of Price, it is said, that he was a striking figure in the formative days of surgery for pelvic disease, and was a great force, radiating his new doctrines far beyond the confines of Philadelphia. His forceful arguments and appeals were hammer blows upon the anvil of progress, and the flying sparks reached the remotest corners of the surgical world, for his teachings helped to bring about the perfected and finished technique we now enjoy, with little thought of the hours of plodding that produced it.[1]

Price recognised that Tait was a master and he wisely followed him, and as Price was a masterful spirit who was not afraid to drive his views home, he soon convinced many that Tait's views were the right ones, and it was wise to follow him. Price always acknowledged his indebtedness to Tait and never tried to pose as the originator of methods that he had copied from his teacher.

The surgery of the Fallopian tubes being now an acknowledged branch of surgery, and their removal being now designated "Tait's operation," each year brought more American surgeons to Birmingham to see Tait's work. After a time this curiosity began to upset the Birmingham surgeon and he did not always receive the pilgrims with open arms, and at last almost refused their admittance.

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[1] *Amer. Jour. Obst.*, October, 1918, p. 606.

Those who were privileged to see his work returned to America and read reports and published papers on what they had seen. These papers throw a light on Tait's methods, and give us some idea of the estimation that his work was held in at this period.

The first extract that we shall give is by Van der Veer, at whose invitation Tait visited America. The paper is entitled:

SOME PERSONAL OBSERVATIONS ON THE WORK OF LAWSON TAIT.[1]

I am free to confess that seldom have I had so strong and favourable an impression made upon me by any stranger as I received in meeting Mr. Lawson Tait at his private hospital in Birmingham, England.

A short conversation convinced me of being in the presence of an original thinker, and later, when the opportunity to see him operate was given, there was ample proof presented of his being a careful, cautious, great surgeon.

His book on "Diseases of the Ovaries" I had read and studied with so much benefit that I was somewhat prepared to grasp the hand of a practical worker, but in no sense did I fully understand the acuteness of his investigations, the ingenious methods of operating he possesses, and the far-reaching scope of his present work.

His hospital is a model of all that one could wish regarding quietness, cleanliness, and perfect system, not only in nursing, but in everything. The discipline is the outgrowth of years of hard work and close application, and yet Mr. Tait is scarcely forty years of age.

He has described quite fully in his book the necessary preparations incident to an operation, and yet to witness it all is a study. Everything is arranged by the nurses after being told of the nature of the operation and the hour of operating.

He selects young, intelligent and prepossessing women, whom he trains for the work he so much enjoys, and they in their desire to learn and please become true enthusiasts in the struggle to save life. He will not have morose, untidy, or homely women as nurses.

Mr. Tait enters the room, and almost at a glance tells whether all required instruments are selected. He brings his bag of carefully prepared sponges, and, counting them again, tells the assistant nurse their number, and she is held

[1] *Amer. Journ. Obst.*, July, 1885.

responsible for them. So also in regard to the number of forceps and other instruments.

The following is his method of preparing the sponges, and but one person is trusted to do this.

New sponges are put first into a large quantity of water with sufficient muriatic acid to make the water taste disagreeably acid.

They remain in this mixture until all effervescence has ceased and all the chalk is removed. For this purpose it may be necessary to renew the acid several times.

The sponges are afterwards carefully and thoroughly washed to make them as clean as possible and free from every rough particle.

After being used at an operation, they are first washed free from blood, and then put in a deep jar and covered with soda and water (one pound of soda to twelve sponges). They are left in this about twenty-four hours (or longer if the sponges are very dirty) and then they are washed perfectly free from every trace of soda.

This takes several hours' hard work, using hot water, squeezing the sponges in and out of water and changing the water constantly. Leaving them to soak for a few hours in very hot water greatly assists in the cleansing.

When quite clean they are put in a jar of fresh water containing about 1 per cent. of carbolic acid, and after being in this for twenty-four hours they are squeezed dry and tied up in a white cotton bag, in which they are left hanging from the kitchen ceiling (being the driest place in the house) till they are wanted.

His hospital bears every evidence of good plumbing and careful ventilation. No extra furniture is allowed in any room. Rugs in place of carpets are used, except in the halls and on the stairs, where carpets with matting may be seen. The rooms, by reason of brick partition-walls, are exceedingly quiet. There are beds for twenty-two patients, and these are kept constantly filled. (He is now building a large addition for greater accommodation.) Aside from true gynæcological work, he occasionally does other surgical operations, of which I shall speak later.

Rooms are thoroughly cleansed after each patient leaves, and no display is made of any particular antiseptic in so doing. Nurses and attendants go about with cheerful faces, but no noise. Mr. Tait himself, when going through the rooms visiting, makes but few remarks aside from what is required in dictating treatment, etc., and although a man who enjoys a well-told story and general conversation, indulges in no talk here. I was strongly impressed with the fact that his own dwelling-house adjoins his hospital and communicates with

it, and when a case goes badly he is promptly on hand, trusting to no one else to right matters.

In his operations he has a lady physician to administer the anæsthetic (his preference he has already told us in a recently printed address), and one male assistant, whose position is directly opposite him and yet who is seldom allowed to handle any of the sponges or instruments, only assisting, when the abdominal walls are being opened and in ligating the pedicle, appendages, or adhesions, depending upon the nature of the operation and its complications.

He operated standing at the right side of his patient and has his instruments and sponges on the same side within easy reach.

Patient is placed upon a narrow table, legs and arms fastened in the ordinary manner, but no rubber protective used.

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The second extract is from a paper by Paul F. Mundé,[1] entitled :

A Glimpse of Laparotomy in Europe[2]

On receipt of a note from Mr. Tait that he would do two laparotomies on the next day, I went to Birmingham, and had the good fortune to see him do three abdominal sections in one day.

So much has been written about Mr. Tait's peculiar methods that I shall not repeat what is probably well known to those of the profession interested in laparotomy, but will merely give a very brief sketch of what I saw.

Tait operates both in his private hospital and a small public institution in a suburb of Birmingham. He operates in the sleeping-room of the patient, uses absolutely no antiseptics, either for hands, instruments, sponges, or about the room, which is papered, hung with pictures, and small, filled bookshelves, etc. On being notified that the patient is under ether (by Clover's inhaler, which I have used exclusively for about three years), Tait proceeds to the room, removes his coat, strips up his sleeves, washes his hands and arms, puts on an apron, takes a knife from a pocket-case lying on the window-sill, tries its edge on his thumb, and makes the incision. A lady gives the ether; a male assistant (who

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[1] Mundé was Professor of Gynæcology at the New York Poleclinic, and the editor of Gaillard Thomas' great work 'A Practical Treastise on the Diseases of Women."

[2] *Amer. Jour. Obst.*, September, 1886.

does little but aid in tying the pedicle ligature) stands opposite Tait, and there are two or three female nurses in the room to hand sponges, etc. Everything is done noiselessly, quickly, and systematically, without any ceremony or fuss. The patient lies stretched her whole length on a plain table. The instruments used are a knife, a number of hæmostatic forceps, several long serrated forceps, perhaps a trocar. The pubes is not shaved or washed by the operator before making the incision, which is very short, not more than one to one and a half inches in length. Lifting up the tissues with two forceps, one of which is held by the assistant, Tait rapidly divides layer after layer until the peritoneum is reached, which he seizes and lifts up to the level of the skin, nicks, and at once enlarges the opening by thrusting in the two first fingers of his left hand into the incision, which they completely fill. Now his wonderful dexterity and tactile sense come into play, for with these fingers he at once makes the diagnosis (which he appears to pride himself on not attempting to make with accuracy in those cases which call for removal of the uterine appendages, the so-called "Tait's operation," except through the abdominal incision), peels off the organs if they are adherent, and in a trice brings them out through the incision. With a straight Peaslee needle, he transfixes the pedicle, slips the noose of the ligature over the ovary and tube, and ties the "Staffordshire knot" always used by him, which is very simple to understand when seen, but less easy to describe or to comprehend from a description. The knot being securely tied and the ends cut short, the ovary and tube are cut off with scissors, and the stump dropped without more ado. The same on the other side. A sponge catches whatever oozing there may be from adhesions, the small incision is closed by two or at most three silk sutures, a small pad of absorbent cotton is placed over the wound, which is so small as to appear merely like a faint red line, and the operation is concluded.

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In a paper in the *New York Medical Record*, Dr. Mary Dixon Jones, in 1886, says: "There is no advance made in modern surgery that will do more good or save more lives, or relieve more suffering, or add more to the sum of human life or human happiness than this one operation, known as 'Tait's operation.' It will save more lives than ovariotomy because more need it."

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The last extract is from a paper by R. T. Morris entitled :

The Reasons for Tait's Success in avoiding Septic Accidents [1]

The central point in the paper was in the statements that the same number of microbes, when introduced into the knee-joint, would produce disastrous inflammation, but when introduced into the peritoneal cavity might not produce unpleasant consequences because of the extensive absorbing surface and the ability of the emunctories to carry them away.

If decided cleanliness, therefore, be observed, the risk of introducing microbes into the peritoneal cavity was small, and the giving, as Mr. Tait does, a cathartic on the fourth or fifth day acts like an antiseptic and carries the septic material out of the system through the emunctories whither it has found its way. In other words, by the bowels was removed the septic infection which, if allowed to remain, would develop into serious, if not fatal septicæmia.

Dr. Polk regarded the main drift of the paper as very interesting, especially to those who had practised the method with such results as to lead them to commend it highly.

But there was another point which to him had been equally interesting. Take the case of a woman who began to show signs of septicæmia, and she was purged, but the purgation was not sufficient to arrest the disease and the patient died. The autopsy revealed a focus of infection in one of the tubes. If the woman had had proper drainage in addition to purgation it was possible that she might have been saved. He would say that purgation was not to be substituted for drainage at all. It was to be employed in the milder cases of septic infection, whether drainage was effected or not ; but it was not to come in competition with drainage.

The plan of treatment by purgation was an old one, and the results obtained in the old times were not satisfactory. It proved to be a failure, as was shown by the success which attended the plan of treatment laid down by Dr. Alonzo Clark. But now when the abdominal cavity was opened and washed out, we had the ideal treatment of peritonitis.

Mr. Tait does not permit his patients to have opium, even though they have considerable pain, and Dr. Polk thought it was a very good point. But where there was peritonitis, he had not been able to rid himself of the opinion that opium was an excellent remedy ; indeed, better than any other remedy at our disposal.

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[1] *The Medical Record*, June 4, 1887.

Dr. Wylie said that the subject was one in which he had been very much interested. So far as he knew, Mr. Tait was the first to revive the idea. But Dr. Wylie had been studying and practising from a somewhat different standpoint than had Mr. Tait. Mr. Tait had succeeded because he had used purgatives early. Dr. Wylie was satisfied that if they were given early the success would be very great; whereas, if delayed, they would do no good. His own view was that the symptoms present at the time when the purgatives were administered, such as eructation, nausea, etc., were due to *obstruction of the bowels*, and that relief followed because of the removal of the obstruction rather than because septic germs were carried off. He believed that Dr. Morris was wrong in assuming that septic germs enter the peritoneal cavity, because Mr. Tait operates so rapidly and through so small an opening, which was filled by his fingers, that literally nothing got into the cavity. He also thought that Mr. Tait's success had increased since he used the drainage-tube, the object of the tube being really to keep the peritoneal cavity empty and thus promote the glazing over of the bleeding-points on the peritoneal surface, which would not take place if there was serum there. Besides, the drainage-tube was so serviceable for detecting hæmorrhage.

Dr. Mundé said it seemed to him that the reason of Mr. Tait's success lay in the rapidity with which he operated, the extreme cleanliness which he observed, and opening the cavity with a small opening just sufficient to admit two fingers. Under such dexterous operating he was unable to see how any germs could get into the peritoneal cavity. Perhaps his success in other fields was not so much greater than that of others; but in his own operation he had surpassed by far all others, and Dr. Mundé could not help believing that Mr. Tait was so successful chiefly because he did so little injury in the peritoneal cavity.

He regarded purgatives as very good, given at the proper time, but he did not believe that they could be called an antiseptic measure. He did not believe that germs are carried off in this manner. He always felt, when he had put in a drainage-tube, that he had added another element of danger, although in proper cases it was a necessary and inevitable evil, and in cases of doubt he would introduce a drainage-tube.

Dr. Weir thought that the real question was to explain the success of Mr. Tait in *preventing the extension of septic infection*. He also, in common with others, believed that dexterity had much to do with the success of Mr. Tait; but, at the same time, it must not be overlooked that he was cleanly to the last degree, that he was practising what later observations had

shown to be very good *antiseptic* treatment, and that he himself guards his sponges very closely. But when Dr. Weir saw him pour, from a pitcher, water into the abdominal cavity, he was unable to explain how germs were excluded. But it was doubtless true that not every germ that entered a wound created a disturbance ; that an appreciable quantity of germs is required to produce suppurative action. These facts enabled him to get at something of the reasons of Mr. Tait's success. Another point was that he was not only dexterous in operating, but he was also wonderfully expert in meeting emergencies.

Dr. Wylie said that, looking at the question from his own standpoint, he thought that Mr. Tait and Mr. Bantock used antiseptics almost as completely as either he or any one present did. Above all, Mr. Tait succeeded because he had a large brain. But with that he took the greatest pains not only to keep his house clean, but the body and bed-linen of his nurses and patients clean, and his beds could be said to be almost literally a Lister dressing. There was asepsis, if not antisepsis, and practically they both "got there just the same."

Dr. Biggs thought that Dr. Morris was wrong in his premises in the assumption that Mr. Tait has to deal with septic infection under the circumstances mentioned. He believed that all workers in bacteriology appreciated the fact that in the atmosphere of most rooms there were germs, but that they were of little danger, and that even in hospitals there was but little danger of germs entering the operation-wound when proper precautions were exercised. Watson Cheyne had shown that certainly the production of disease was in direct proportion to the number of germs introduced. But the absorbing power of the peritoneum was so great, and the surface was so large, that the number of germs that affected it was comparatively small. And the number of germs introduced by pouring in water, even though it be not sterilised, was very inconsiderable indeed. Boiling rendered water perfectly aseptic, and if continued long enough would kill all the spores. A comparatively large quantity of germs was required to produce even such a disease as erysipelas. As regards the action of purgatives as antiseptics, it seemed to him questionable whether purgation would carry off germs.

Dr. J. E. Weeks expressed the belief that Mr. Tait's methods were essentially antiseptic. In water the number of germs was very slight, and according to his observations, a large number of micro-organisms were required to begin the suppurative process. He believed that but very little of the good effect produced by purgatives could be ascribed to their power to carry off germs.

Dr. Morris, in closing the discussion, said several had remarked that purgatives could not carry off germs, and that they simply acted upon the emunctories. That was the point which he wished to make, namely, that bacteria were eliminated by the emunctories, and that these were stimulated by purgatives.

True it was that a certain number of bacteria were required to produce suppuration, etc., and the peritoneum could effectually rid itself of a large number, but that they were of importance could be made apparent by the fact that the same number when put into the knee-joint would probably kill the patient or necessitate amputation.

XXV

Ovariotomy

1886

In 1886 Tait published a paper on a long series of ovariotomies without a death. The paper is important from more than one point. In the first place, it was a good argument that Tait's aseptic technique was sufficient with regard to this particular operation, and it was Tait's answer to those who maintained that Listerism was necessary to successful results in abdominal surgery. Spencer Wells had, a few months before the publication of Tait's paper, issued his work on the "Surgical Treatment of Abdominal Tumours," [1] and was quite jubilant that he had reduced his mortality from thirty-four in the first hundred to eleven in the last. Tait's reduction from nineteen deaths in his first fifty in 1878, to no deaths in 137 consecutive operations at this period, were results which he did not fail to use against Spencer Wells, who had at this time become his bitter antagonist.

With regard to the operation of ovariotomy, we do not propose to go into the minute steps in its history; that has been so well done by Lee,[2] Peaslee,[3] Spencer Wells,[4] and Tait,[5] that it would serve no good purpose to go minutely over the well-known facts. We propose to devote some little time to a consideration of certain points in connection with the rise and progress of ovariotomy that Tait took an especial interest in.

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[1] Churchill, London, 1885.

[2] "Clinical Reports of Ovarian and Uterine Diseases," Lee, London, 1852.

[3] "Ovarian Tumours," E. Peaslee, London, 1873.

[4] "Diagnosis and Surgical Treatment of Abdominal Tumours," 1885; "Diseases of the Ovaries," 1872.

[5] "The Pathology and Treatment of Diseases of the Ovaries," Birmingham, 1883.

Some of these points were: Who was the first ovariotomist? Who was the "Father of Ovariotomy," as far as Europe was concerned? The influence of Baker Brown, Thomas Keith and Spencer Wells on the operation; and the influence of the antiseptic technique on ovariotomy.

Who was the First Ovariotomist?

This was a subject upon which Tait was fond of dilating. He was firmly convinced that Robert Houston, of Glasgow, was the first ovariotomist. He did not wish to detract from Ephraim McDowell's magnificent work, for he had the greatest admiration for the calm masterly way in which that surgeon had set about his first classical operation. But Tait had a very keen regard for his countrymen, and he would have been proud if he could have proved to every one's satisfaction that Robert Houston, of Glasgow, had been the first ovariotomist. We shall bring forth the evidence as clearly as possible and we shall attempt to show that Tait was quite justified in advocating Houston's claims.

Tait in 1883, in the fourth edition of his work on "Diseases of the Ovaries," and again in a special paper in the *Birmingham Medical Review*,[1] maintains that Houston was the first ovariotomist. Greig Smith,[2] in a paper contributed to the *Practitioner*, took up the position that "although Houston incised and evacuated the contents of an ovarian growth, there is no proof that he performed ovariotomy." It was in answer to this paper that Tait wrote his reply to the *Birmingham Review*.

Greig Smith, in his paper, says that, "not by all authorities is it admitted that Ephraim McDowell was the first to perform ovariotomy. For Robert Houston this honour was claimed by Washington Atlee in 1849; it was admitted by Spencer Wells in 1872, and it has been specially insisted upon by Lawson Tait; and quite recently by Houston's fellow-townsman, Dr. James Finlayson." [3]

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[1] "Was Robert Houston, of Glasgow, the First Ovariotomist?" *Birmingham Med. Rev.*, May, 1897. Tait spells his name Houstoun in some earlier writings.

[2] The *Practitioner*, March, 1897.

[3] Janus, November, December, 11, 1896.

The case that Houston operated on was in brief thus: the patient was forty-five years of age, and

> for ten years before the doctor had seen her she was extremely uneasy, her belly grew very large, and a difficulty of breathing continually increased upon her, insomuch that for the last six months she had scarcely breathed *at all*, but with the utmost difficulty. . . . This tumour was grown to so monstrous a bulk that it engrossed the whole left side from the navel to the pubes. . . .
>
> The doctor, encouraged by the unexpected resolution and importunity of the patient, with an imposthume [1] lancet, laid open about an inch; but finding nothing issue he enlarged it two inches, and even then nothing came out but a thin yellowish *serum*, so he ventured to lay it open about two inches more. He was not a little surprised, after so large an *aperture*, to find only a glutinous substance stopping this orifice; all the difficulty was to remove it; he tried his probe and his fingers, but all in vain; it was so slippery that it evaded every touch, and the strongest hold he could take of it. He took a strong firm splinter, about the end of which he wrapped some loose lint, and thrust it into the wound, and by turning and winding it, extracted upwards of two yards in length of a substance thicker than any jelly, or rather like glue—glue that is fresh made and hung out to dry; its breadth was upwards of ten inches. This was followed by nine full quarts of such matter as I observed in *steatomatous* and *atheromatous* tumours, with several hydatids of various sizes containing a yellowish *serum*, the least of these bigger than an orange, with several large pieces of membrane which seemed to be part of the distended *ovarium*. Then he squeezed out all he could, and stitched up the wound in three places, almost *equidistant*. He was obliged to make use of Lucatellus's Balsam, with which he covered a pledget the whole length of the wound, and over this he laid several compresses dipped in warm French brandy . . . he kept in the lower part of the wound a small tent, which discharged some serosities at every dressing for four or five days.
>
> She mended apace to the admiration of everybody, and lived in perfect health from that time, viz. August 1701 till 1714, when she died after ten days' illness.

Greig Smith says:

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[1] An imposthume lancet is one used to open an abscess. Imposthume is a corruption of aposthume from apostema (ἀποστῆναι, to depart from) an abscess.

> Now in considering Houston's case, we must be certain of the meaning of the words employed. The case undoubtedly records an operation of ovariotomy in the literal sense of the term, incision of an ovary, or ovarian growth. What is claimed from him is that he performed ovariotomy in the usual sense—ovariectomy, as it might be called more accurately—or excision of an ovary or ovarian growth. For this last operation two things are essential; one, that the tumour should be delivered from the abdomen; and the other, that its connection with the body should be severed. It is unnecessary to go beyond these two essentials; and there is not even a suggestion that either was carried out.

Tait's reply to this last objection urged by Greig Smith is as follows:

> But these are not essentials, for it is not always either possible or necessary to deliver the tumour from the abdomen, as I have already shown; and in some cases there is no connection at all with the body, as I have also shown. Further, the very details given so accurately and effectively by Houston show that his was a case where Dr. Greig Smith's essentials were quite unnecessary and wholly unpracticable. In fact, they are not essentials at all in a very marked group of cases, of which Houston's was one, the first on record. Therefore it is that there is not a suggestion in Houston's record that either was carried out. Therefore it is that I made a mistake when, in 1883, I said, "He must have seen and divided the pedicle." I was then as much in the dark as Dr. Greig Smith still is, and like him, I believed in the "essentials," but it is just because Houston so accurately describes what he did do, and says nothing about what he did not do, that his story is absolutely deserving of credence, and why he deserves the credit, be it worth what it may, of being the first to do an intentional complete and successful ovariotomy.

Tait gives the main features of the group to which Houston's tumour belongs. He says, the main features of the group are the singular cyst contents, the rupture of the cyst walls, the extrusion of the contents, and the consequent activity of the peritoneal serous surface; the destruction of the cyst and the evidence of its torn detritus in the cavity of the peritoneum; and, generally, an exceedingly vague area of attachment, and marked absence of prominent blood vessels.

He concludes his paper with these remarks:

> It was only after a comparative leisure that came to my life by the discontinuance of hospital work that I began to think it worth while to stitch together casual notes and recollections, and the re-reading of Houston's vivid description fixed the group in my mind at once.
>
> I said in 1883 he must have tied the pedicle. But I know now that he did not tie the pedicle, because he does not say so, and that the pedicle did not need tying. He had stumbled across one of these "digesting" cases. The tumour was ruptured, and the contents were being extruded into the peritoneal cavity. He opened the abdomen, and was not a little surprised, after so large an aperture, to find only a glutinous substance stop up the orifice; all the difficulty was to remove it; he tried his probe and fingers, but all in vain; it was so slippery that it eluded every touch and the strongest hold he could take of it. No words of mine could excel or improve this description of the extraordinary stuff. "He took a strong fir splinter, about the end of which he wrapped some loose lint, and thrust it into the wound, and by turning and winding it, extracted upwards of two yards in length of a substance thicker than any jelly, or rather like glue that is fresh made and hung out to dry; its breadth was upwards of ten inches. This was followed by nine full quarts of such matter as is observed in steatomatous and atheromatous tumours with several hydatids of various sizes containing a yellowish serum, the least of these bigger than an orange, which seemed to be part of the distended ovarium." Of course these hydatids were secondary cysts. He closed the wound in the method of his time, and (Dr. Greig Smith has not noticed) he drained the peritoneum. "He kept in the lower part of this wound a small tent, which discharged some serosities at every dressing for four or five days."

What conclusion can we arrive at after reading Greig Smith's paper and Tait's reply? We have no hesitation in saying that we must admit Tait's contention that Houston's operation was the first successful ovariotomy. The reason why it has not been more generally accepted is that the tumour was not lifted up out of the peritoneal cavity, and the pedicle tied. But Tait's special knowledge has shown us that this was a rare kind of ovarian cyst, a variety that he had seldom encountered and a kind that few of us have had any experience of. And it is because of its extreme rarity that we are inclined to reject Houston's claims. But this is unjust to Houston, and it is quite within the bounds

McDOWELL PERFORMING HIS FAMOUS OPERATION (OVARIOTOMY) ON MRS. CRAWFORD.
This was drawn from memory by Dr. Witherspoon, who was present at the operation.
From A. H. Barkley's article on Ephraim McDowell in *Surgery, Gynæcology, and Obstetrics*, by kind permission of the proprietors.

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of possibility that Houston's claims will never be generally acknowledged, for, rightly or wrongly, McDowell's operation has come to be regarded as the first ovariotomy, and it will be a difficult matter to try to deprive him of an honour that he seems to have gained without any show, and yet so dramatically that there is nothing finer in the history of surgery.

Let us see what he did. The tale has been told so often that it is almost trite, yet it is so splendid in its simplicity that it will bear repetition for many a decade still.

First of all a few words about the man. When the history of modern gynæcology is written, the work that McDowell did will be represented as the dawn; and the first bright planet that appeared in the dim light of that dawn was Marion Sims, whose light came to shine more brightly than that of any other star of the first magnitude in later days.

Ephraim McDowell (1771–1830) was born in Virginia,[1] on March 11, 1771. His ancestors came from Scotland in 1737, and he, after attending school at Georgetown, Kentucky, went to Scotland, in 1793, to attend lectures at the University, Edinburgh. He remained there a year, and during that time he attended the private instructions of John Bell, the first surgeon of the day.

> That portion of Bell's course in which he lectured upon the diseases of the ovaries and depicted the hopeless fate to which their victims were condemned, made a powerful impression upon his auditor. Indeed, McDowell afterwards stated that the principles and suggestions at the time enumerated by his master, impelled him, sixteen years afterwards, to attempt what was considered an impossibility.[2]

McDowell returned to Danville, then a small village in the western wilderness, and began practice, and soon he was performing every kind of surgical operation, "but he never operated without consulting the best surgical thought." [3]

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[1] Tait has written ("Diseases of the Ovaries"), "My American readers may object that McDowell was not born in Scotland; of this, however, we are not clear. At any rate, his father and his mother were Scotch."

[2] "A Cyclopedia of American Medical Biography," Howard Kelly, vol. ii. p. 128.

[3] "Some Facts not generally known regarding Ephraim McDowell," A. H. Barkley, *Surgery, Gynæcology, and Obstetrics*, vol. xv. p. 496, 1912.

In 1809, fourteen years after he began practice, he was sent for to see Mrs. Cranford, living in Green County, Kentucky. Her medical attendants thought that she was pregnant, but McDowell announced that the patient had an ovarian tumour, which was rapidly growing and hastening to a fatal termination. As Gross says :

> After a most thorough and critical examination, Dr. McDowell informed the patient, a woman of unusual courage and strength of mind, that the only chance for relief was excision of the diseased mass. He explained to her, with great clearness and fidelity, the nature and hazard of the operation ; he told her that he had never performed it, but that he was ready, if she were willing, to undertake it, and risk his reputation upon the issue, adding that it was an experiment, but an experiment well worthy of a trial.[1]

At the close of the interview, Mrs. Cranford said that any mode of death was preferable to the slow death that she was undergoing, and she was quite willing to submit to the operation. We cannot help admiring the calm courage of this woman, who was forty-seven years of age, and she well deserved to live to the ripe age of seventy-eight years, dying on March 30, 1841. McDowell promised to perform the operation if the patient could come to Danville, the town where he lived, which was at a distance of sixty miles from the residence of the patient. But this suggestion seemed fatal ; yet the brave patient, being determined to submit to the operation, made the journey on horseback.

And now let us come to the operation itself. McDowell was assisted by his nephew, James McDowell, and the operator has left us the following particulars :

> Having placed her on a table of the ordinary height, on her back, and removed all her dressing which might in any way impede the operation, I made an incision about three inches from the musculus rectis abdominis, on the left side, continuing the same nine inches in length, parallel with the fibres of the above-named muscle, extending into the cavity of the abdomen, the parieties of which were a good deal contused, which we ascribed to the resting of the tumour on the horns of the saddle during the journey. The tumour

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[1] O. Gross, " Lives of Eminent American Physicians," Phila., 1861.

HOUSE IN WHICH McDOWELL PERFORMED HIS FAMOUS OPERATION ON MRS. CRAWFORD WITH SUCCESS.

This house is still standing in Danville, Kentucky.

From A. H. Barkley's article on Ephraim McDowell in *Surgery, Gynæcology, and Obstetrics*, by kind permission of the proprietors.

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then appeared full in view, but was so large that we could not take it away entire. We put a strong ligature around the Fallopian tube near to the uterus; we then cut open the tumour, which was the ovarian and fibrous part of the Fallopian tube much enlarged. We took out fifteen pounds of a dirty, gelatinous looking substance; after which we cut through the Fallopian tube and extracted the sac, which weighed seven pounds and a half.

As soon as the external opening was made, the intestines rushed out upon the table, and so completely was the abdomen filled by the tumour, that they could not be replaced during the operation, which was terminated in about twenty-five minutes.

We then turned her upon her left side so as to permit the blood to escape, after which we closed the external opening with the interrupted suture, leaving out at the lower end of the incision the ligature which surrounded the Fallopian tube.

Between every two stitches we put a strip of adhesive plaster which, by keeping the parts in contact, hastened the healing of the incision. We then applied the usual dressing, put her to bed, and prescribed a strict observance of the anti-phlogistic regimen.

After reading the account of the operation we hardly know whether to admire more the superb courage of the patient, who determined to have the operation, rode some sixty miles, and endured the pain of that long journey, which showed itself in the tissues of the parietes " which were a good deal contused, which we ascribed to the resting of the tumour on the horn of the saddle during the journey." We hardly know, we repeat, whether to admire more the courage of the brave woman or the confident skill of the country surgeon.

When we think of one living on the border of Western civilisation, in a little town of five hundred inhabitants, far removed from the opportunity of consultation with any one whose opinion might be of value, and nearly a thousand miles from the nearest hospital or dissecting room, performing a new and untried operation of such magnitude upon the living, before the days of anæsthesia, with a full sense of the responsibility and danger, without skilled assistants, our admiration for McDowell's courage and skill rises to its full height. (McMurtry.)

It is also well to remember that McDowell had to display some courage after the performance of the operation, for " while the operation was being performed, there had

assembled in the street in front of his office an angry mob to do violence to McDowell, who was ' butchering a woman ' inside " (Barkley).

A word in conclusion concerning the after-course of this operation ; the fact so accords with the views of some of the so-called advanced surgeons of the day, they no doubt will hail it with delight.

McDowell says, " In four days I visited her, and much to my astonishment found her engaged in making up her bed." The fact will no doubt please those surgeons who delight in having the patient up on the third day after a section !

It must not be supposed for a moment that if Tait tried to have Houston's claim acknowledged that he did not appreciate the merit of McDowell's achievement. He said that McDowell's grand-daughter had gone so far as to class him on the list of those who had attacked her illustrious ancestor, because he had indicated an interest in Houston. But Tait never wished to detract from McDowell's great achievement, and often when speaking of the first ovariotomy in America, Tait had nothing but words of admiration for the American surgeon. He has written : " Ephraim McDowell has been honoured by the medical profession in America as the ' Father of Ovariotomy,' and whether we admit the accuracy of the title or not, there can be no doubt that it was in the backwoods of Kentucky that abdominal surgery received one of its greatest impulses."

Goffe has claimed that McDowell instituted the following points in the technique of ovariotomy :

1. In the first case, and in five others of his series, he made his sweeping long incision, laying open the abdomen from the border of the ribs to the spine of the pubis, sometimes at the outer border of the rectus, sometimes in the median line. In two cases, the third and the sixth, he used the short median incision below the umbilicus.

2. The principle of regarding the short incision as exploratory, inserting one finger or hand for exploration in diagnosis, holding in reserve the practicability of enlarging the incision and completing the operation, or puncturing and draining the cyst when removal was impossible.

3. The practice of avoiding the umbilicus in extending the incision and going around it to the right or left.

4. The practice of turning the patient upon her side to prevent the fluid getting into the peritoneal cavity or emptying it when it had escaped into it.

5. The principle of transfixing the tissue of the pedicle with the ligature to prevent slipping. This he applied after the slipping of the ligature in one of his cases. In this case and in one other he cut the ligature short and dropped the pedicle into the abdominal cavity.

6. The closure of the wound with interrupted sutures, together with broad adhesive strips, and the application of compress and abdominal binder.

Tait has also drawn attention to the fact that McDowell did not publish the results of his first, and his subsequent, ovariotomies until 1817, and that his results were received with general incredulity. The editor of the *British and Foreign Medical and Chirurgical Review* was bold enough to distinctly deny the credibility of McDowell's statements; but in 1827, when the authenticity of the accounts had been established beyond doubt, he had the manliness to " beg pardon of God and of Dr. McDowell of Danville " for his hardiness.

Another point in connection with ovariotomy that Tait was fond of speaking about was that most of the early cases that are recorded as instances of ovariotomy, were in reality cases of removal of *parovian* tumours. He said that, in 1822, Nathan Smith, of New Haven, performed an operation successfully. Nathan Smith's case was undoubtedly one of parovian cyst, and the operation was, therefore, not an ovariotomy at all. It is notable, however, from the fact that he used the short incision and the short ligature. " The curious fact," he remarks, " that so many of these early successful cases were parovian tumours makes me believe that a great many more true ovariotomies were done, of which there are no records, for they probably all died."

Tait draws attention to the fact that in 1842 Walne performed three operations, all of which were successful, and he published them as ovariotomies; but Tait says that

" singularly enough, in not one of these cases was the tumour removed an ovarian cystoma. ' Mr. Walne gives the figures of the first of his cases, and he so thoroughly describes the appearances of the other two that no doubt can exist that they were parovian cysts, and it is not clear whether he did or did not remove the ovaries with them."

Tait in another place, in alluding to Charles Clay's operations, says that he had on September 27, 1842, performed his first ovariotomy, and he had on the twelfth of the same month performed another operation; but here again there is abundant evidence to show that a parovian was mistaken for an ovarian tumour. Previous to September, 1842, we have, therefore, records of only two ovariotomies, properly so called, in this country, those of Houston and Lizars.

Another point that Tait was very keen on was that Charles Clay, of Manchester, should be regarded as the " Father of Ovariotomy," as far as Europe was concerned. He said this in 1883 (" Diseases of the Ovaries "), and in the following year [1] he repeated this statement in a letter to the *British Medical Journal:*

> I have just heard with deep regret, that Dr. Charles Clay, of Manchester, at the age of eighty-three, is stricken down with paralysis. I therefore hasten to take up a matter which concerns him, namely, the claim made by Sir Spencer Wells concerning the revival of ovariotomy.
>
> Sir Spencer Wells' address, recently delivered before the Midland Medical Society, was announced with the brief title, " Modern Surgery," but on its appearance in print this title is enlarged to " An Address on the Revival of Ovariotomy, and its Influence on Modern Surgery," and in it he practically makes a claim of having revived ovariotomy between the years of 1858 and 1865. Such a claim as this cannot, in the least degree, be substantiated when the facts of the case are fully examined.
>
> So far as has yet been discovered, the first ovarian tumour removed in England was by the hands of Dr. Charles Clay, on September 27, 1842; all others, with the exception of one by Houston, and another by Lizars, in Scotland, were clearly parovian cysts. In 1843, Mr. Aston Key removed both ovaries, and Mr. Bransby Cooper also tried the operation in that year, but it was not till 1844 that there was a successful

[1] " The Revival of Ovariotomy," *Brit. Med. Jour.*, December 6, 1884.

case in London, operated on by Dr. Frederick Bird, followed by one in the practice of Mr. Lane. In the provinces, however, many successful cases had been done. In June, 1848, Dr. Charles Clay published a series of thirty-two cases with ten deaths, and in 1857 he had completed seventy-seven cases with twenty-four deaths, and he continued to operate for many years, until he had performed three hundred and ninety-five operations, with one hundred and one deaths, his total mortality being practically above 25 per cent.—a mortality which I may remind Sir Spencer Wells is almost identical with that (25 per cent.) of his own 1000 cases. At the same time the operation was being performed frequently in the large provincial towns of England, but particularly in Manchester, by Southam and others, with great success.

The only revival, therefore, of ovariotomy, which Sir Spencer Wells can lay any claim to, is after its temporary decadence of two and a half years in London in the hands of Mr. Baker Brown; and even upon this point the evidence is by no means satisfactory. But if it is to be contended that, from the time of McDowell till 1857, there was nothing being done in ovariotomy, and that the revival took place in that year at the hands of Mr. Spencer Wells, I say it may as well be claimed for him that he revived the moon. To Dr. Charles Clay is due the credit of having established ovariotomy in England, and carried it through its early struggles. His practice was a mistaken one in the use of the long ligatures, but the results were certainly no worse than those obtained by the clamp. Dr. Clay, unfortunately, did not know of the results obtained by Nathan Smith from the use of the short ligature. Had he done so, abdominal surgery would have been half a century in advance of its present position, for then it would have been impossible for the clamp even to have made its appearance.

Baker Brown re-introduced Nathan Smith's principle, and Keith has brought it to perfection.

Of all the points in connection with ovariotomy that Tait took an interest in, that in regard to Baker Brown's influence, after he had re-introduced the short ligature in dealing with the pedicle, was the one that he laid most stress on.

We have already in a former chapter showed how Tait, after the publication of his first fifty cases with its big mortality, was so shocked at his own failure that he at once sought to find the cause, and he found it when he cast aside Spencer Wells' murderous clamp, adopted the short ligature, and returned the pedicle into the peritoneal cavity.

There can be little doubt that it was the knowledge of what Baker Brown [1] had been able to achieve, and the knowledge of what Thomas Keith was achieving, that caused Tait to take the step which at once altered his mortality in ovariotomy and put him on the right path.

With regard to Baker Brown, Tait has written :

> The great improvement which was effected upon Clay's method was that which, curiously enough, had been employed twenty years before by Nathan Smith, but which was neglected until re-introduced and firmly established by the late Mr. Baker Brown, and to this most able, but most unfortunate surgeon, I unhesitatingly award the position of having achieved the second advance in abdominal surgery. He began to operate in 1851, and his career ended in 1867. He established the short incision, the intra-peritoneal method of dealing with the pedicle, the use of the actual cautery for its treatment, and the complete closure of the abdominal wound.

Thomas Keith [2] has testified to Baker Brown's great work, and he has said : " His [Baker Brown's] method of dealing with the pedicle by the cautery at once lowered the mortality to one-half of that with the clamp, and it was becoming practised in London when illness came to him and death."

We now come to a very interesting point in the history of ovariotomy, and that is, " What influence did Spencer Wells exert on ovariotomy, if he did not reduce the mortality of the operation ? "

In order to answer this question we must glance briefly at his career.

Wells was born in 1818, he obtained the Membership of the Royal College of Surgeons of England in 1841, and joined the navy as a surgeon. Having served six years at Malta, he left the navy, and in 1845 proceeded to Paris to continue

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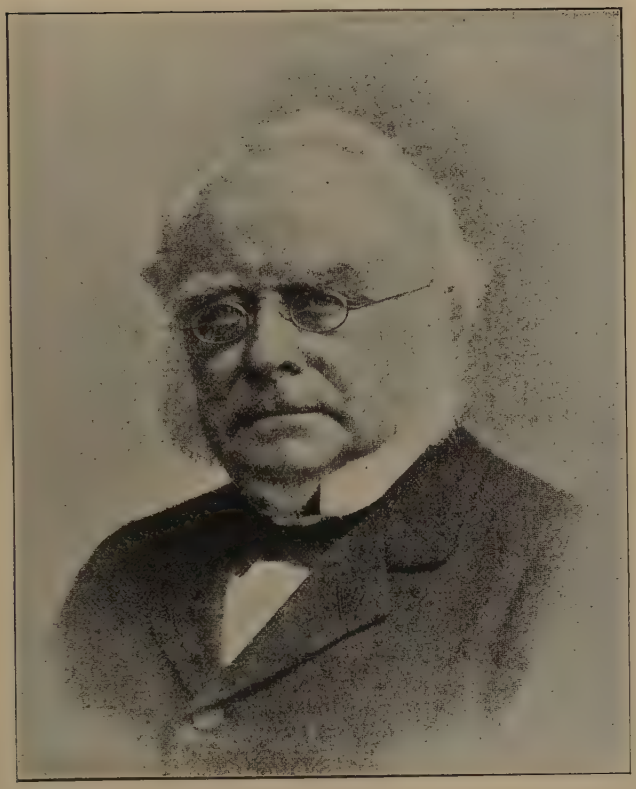
[1] " As soon as it was shown to be less dangerous than had been imagined to cut off the ligature close to the pedicle, and return the whole into the abdomen, I turned my attention to the object of dispensing with this portion of the ligature, and thus making the operation still less dangerous. By means of the actual cautery, I have shown that in many cases the pedicle may be safely divided. Thus I avoid the necessity of introducing a foreign body into the abdomen." Baker Brown, " Ovarian Dropsy," 1868.

[2] *Brit. Med. Jour.*, July 31, 1880.

PLATE XXVII.



SIR SPENCER WELLS.

By kind permission of the proprietors of the *Lancet*.

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his studies. Among Spencer Wells' acquaintances in Paris was Dr. Waters, afterwards of Chester, who first directed his attention to the operation of ovariotomy. The two had many discussions on the operation, and determined, as was almost universally acknowledged in England, that it was an unjustifiable operation.

In 1854 Wells joined the Samaritan Hospital, at that time a dispensary for the diseases of women, " A subject," which Wells afterwards confessed, " of which I knew less than of any other special division of our profession." In 1854 Wells became acquainted with Baker Brown, and in that year he and Nunn assisted Baker Brown in his eighth case of ovariotomy. This, says Wells,[1] " was the first time that I saw the operation attempted, and it certainly did not encourage me to look favourably upon ovariotomy. The patient died, and her death discouraged Brown. He, Nunn, and I talked the matter over, and I well remember Brown saying, ' It's the peritonitis that beats us.' "

When the Crimean War broke out, Wells went to the East with the army. He had an opportunity of attending to many abdominal wounds, which taught him that the peritoneum would bear much rougher handling than he had previously believed permissible.

> I learnt in the Crimean that a man's abdominal wall might be lacerated by fragments of shell, his intestines protruding and covered with mud, so remaining for several hours ; and yet that, after careful cleansing of the cavity and accurate closure of the wounds, complete recovery was possible. When I returned to London, 1856, I was certainly much less afraid than before of abdominal wounds.

Wells now took up his work at the Samaritan Hospital again, and it was not till December, 1857, that he made his first attempt to perform ovariotomy, but this was a complete failure.

He has stated his position at this time in the following clear words :

> I think it would be difficult to imagine a position more disheartening than that in which I was placed when making

[1] " The Revival of Ovariotomy," *Brit. Med. Jour.*, November 8, 1884.

my first trials of ovariotomy. The first attempt, as I have said, was a complete failure, and strengthened, not only in the minds of others, but in my own mind, the fear that I might be entering upon a path which would lead rather to an unenviable notoriety, than to a sound professional reputation. And if I had not seen increasing numbers of poor women hopelessly suffering, almost longing for death, anxious for relief at any risk, I should probably have acquiesced in the general conviction, have been content with palliative tapping, or making some further trials of incision and drainage, or of iodine-injection or of pressure, rather than have hazarded anything more in the way of ovariotomy. It may be forgotten now, but it is true, that at that time everything was against the venture. The Medical Press had denounced the operation, both in principle and practice, in the strongest terms. At the Medical Societies, the speakers of the highest authority had condemned it most emphatically. The example of the men who had practised it was not followed; some of them had given it up. Only once had a successful result been obtained in any of our large metropolitan hospitals, that of Cæsar Hawkins, at St. George's Hospital, in 1846—and he never undertook it a second time. Every other attempt, at Guy's Hospital by Morgan, Key, and Bransby Cooper, at St. Thomas's by Solly, had ended in death. This is how we stood in the year 1857. It was in February, 1858, that I completed the operation for the first time.

From this time on Wells' name became known everywhere in connection with ovariotomy. He tried to be honest, and he pledged himself publicly to make the results of his operations fully known to the profession—to publish every case, whether it was successful or not—and he scrupulously fulfilled this promise. No one doubted his honesty of purpose, and as Greig Smith said years afterwards: "Round his personality centred all the changes, improvements, and we must add, retrogressions, which have followed the fortunes of the operation. His hearty readiness to try the recommendations of others, though it has not always been conducive to reduced mortality, has testified to his honesty of purpose and breadth of view."

Wells continued to operate for years, yet always with a shocking mortality. At the conclusion of his first five hundred cases he had to record one hundred and twenty-seven deaths, *i.e.* 25 per cent.; one in every four cases died.

When in 1877 he resigned from the Samaritan Hospital he had in the twenty years at the hospital performed ovariotomy 408 times with ninety-nine deaths, that is to say, his mortality for the whole series was 24 per cent. !

Tait, in the preface to his work on " Diseases of Women," published in 1889, makes this statement which to many will at first sight sound like an exaggeration :

" In 1877 Spencer Wells left ovariotomy where it had been for half a century before him, with a mortality of one in four."

This statement is quite correct as regards the mortality of the operation as is shown by Wells' own table published in 1885.[1]

Series of years.	Cases.	Deaths.	Recoveries.
First four years (1858 to 1862)	30	9	70 per cent.
Second four years (1863 to 1867)	83	21	74 ,,
Third four years (1868 to 1872)	133	36	73 ,,
Fourth four years (1873 to 1877)	162	33	80 ,,

Thus we see by the table that his mortality was 30, 36, 27, and 20 per cent. ; 24 per cent. for the whole series, and accordingly Tait's statement is almost true.

When at a *later* period Wells adopted antiseptics and gave up the clamp his mortality fell to 10 per cent.

Now, although Wells was not able to reduce the mortality of the operation until others had shown him where he was wrong, he nevertheless had a powerful influence on the progress of the operation on the Continent. For instance, ovariotomy was practically a condemned operation until Nélaton came to England to see Wells operate in 1862, and after witnessing several operations returned to establish the operation in France. Again, Wells was the first to operate in Belgium, and led the way to the practice of ovariotomy in Switzerland.

Among the German operators who first practised the operation we may mention, Nussbaum of Munich, Billroth of Vienna, and Grensen of Dresden ; all these men had come to England to see Wells operate and had returned to

[1] " Diagnosis and Surgical Treatment of Abdominal Tumours," Spencer Wells, London, 1885, p. 59.

their respective countries to advocate and practise the operation.

We are now in a position to give credit to the men who established ovariotomy.

McDowell established the operation in America; Clay established the operation in England; Baker Brown, by his treatment of the pedicle by the cautery, was the first to make these operations a surgical success as regards mortality; Keith, by adopting Baker Brown's cautery method and by antiseptics, made the operation still more safe and reduced the mortality to a small percentage. Spencer Wells; though he did not reduce the mortality of the operation, yet around him centred all the changes, improvements, and retrogressions of ovariotomy. To Koeberlé of Strasburg, and Péan of Paris, French surgeons owe a debt of gratitude, for these men, by the introduction of the hæmostatic forceps, improved the technique of the operation, which was brought by Terrier, under the influence of antiseptics, to the highest perfection. Tait, by his wonderful series of 139 consecutive ovariotomies without a death, put up a record which will probably stand for many years, as it is a very difficult thing to operate on even a hundred consecutive ovarian cysts and not have a death from some cause apart from the operation.

The last point about ovariotomy that we come to is the effect that the use of antiseptics had on the decrease in the death rate of operations on ovarian cysts.

At first sight one would say that the decrease in the death rate was due to the antiseptic methods of Lister. As a matter of fact this is not strictly true. Thomas Keith and Baker Brown, by their technique, reduced the death rate quite independently of Lister. But while this is true of these pioneers, yet for the majority of surgeons Listerism was the key that led the way to success. Tait in his usual obstinate way denied this; let us, therefore, as a contribution to the history of the evolution of ovariotomy, examine the facts and then draw conclusions.

In the first place, it is most important in considering the evolution of abdominal surgery to recognise that ovariotomy

was the operation which enabled abdominal surgery to come into practical existence. The ovarian tumour was the battlefield whereon the first abdominal engagements were fought, and the mortality in this operation remained so shocking, in spite of every plan of treating the pedicle, that it was not until Listerian principles were adopted by surgeons in general that a great reduction in mortality followed. Tait always maintained that antiseptics had nothing to do with the success of ovariotomy; but that was his prejudice and his inability to see that his own asepsis was but an advanced stage of Listerism.

Ovariotomy before and after the Introduction of Listerism

Ovariotomy in America

McDowell, between 1809 and the date of his death, 1830, performed ovariotomy thirteen times, and eight of these operations were successful: mortality, 38·4 per cent.

Previous to the year 1850, twenty-two surgeons had attempted ovariotomy in America, and eighteen had performed it: of the completed operations—thirty-six in number—twenty-one recovered and fifteen died: mortality, 41·6 per cent.

The total number of cases up to 1863 is 117, of which 68 recovered and 49 died: mortality, 41 per cent.

Up to 1871, 660 operations had been performed with 207 deaths: mortality, 31·3 per cent.

Among the most successful operators in America should be mentioned John Light Atlee (1799–1885), who performed ovariotomy eighty-eight times with sixty-four recoveries: mortality, 27·2 per cent.

From 1871 up to 1886—the time we are dealing with—the results improved, although the results of the best operators, "while very excellent, are not quite so good as those of a few of the leading operators in Europe." [1] In recent years, of course, the results have been as perfect as any results ever published.

[1] Goodell in Mann's "System of Gynæcology," vol. ii. p. 792.

Ovariotomy in Great Britain

After Lizars had attempted ovariotomy in Scotland, in 1825, the experiment was not repeated for twenty years. Up till 1862 it had seldom been attempted and had succeeded only in one instance.[1]

In Ireland it had been performed up to 1862 only three times, and all three cases had fatal results.

West, of Tunbridge, in 1836, removed an ovarian tumour, tied the pedicle, and returned it.[2]

The only ovariotomy completed in a London hospital was performed by Phillips in 1840, and the first successful case was done by Cæsar Hawkins in 1845, and there was not another successful hospital case until 1858.

Walne recorded in 1842 his first success in London, and Lane, in November, 1843, operated with success also in London ;[3] and while the operation had never before been successful in London, it had been ten times successful in the provinces, and it must be recorded that it was the provincial surgeon in England that had done most towards placing the operation of ovariotomy on a sure base.

Of all the provincial surgeons, Charles Clay, of Manchester, is the one that stands out as the man who established ovariotomy in Great Britain. He commenced his career as an ovariotomist in 1842, and recorded three recoveries out of four operations during that year. To this surgeon, more than to all the other operators, the credit belongs of having placed the operation of ovariotomy on a sure base : this was the opinion of Peaslee, an American, and of Tait.

Clay had performed the operation six times, and three times successfully before 1844, and up to 1850 he had had twenty-one recoveries out of thirty-three operations ; as many operations as all the other British operators together had performed.

Up to 1866 Clay had operated one hundred and thirty-seven times with forty-two deaths : mortality, 30 per cent.

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[1] The *Lancet*, January 1863, p. 70.
[2] *Brit. Med. Jour.*, December 20, 1884 : Letter by Samuel Willis.
[3] *Brit. Med. Jour.* December 13, 1884.

Up to 1862 the three most experienced operators in Great Britain had had the following results :

Operators.	Cases.	Deaths.	Mortality.
Clay	110	33	30
Spencer Wells	76	23	30
Baker Brown	52	24	46

The whole number of reported cases of ovariotomy in Great Britain up to the beginning of 1864 is 378, with 148 deaths, mortality, 39 per cent.

From 1864 up to 1870, Spencer Wells loomed so largely in the foreground in connection with ovariotomy, that he, in a sense, had a monopoly of the operation, and as he used the clamp he exercised a harmful influence on the progress of the operation.

The following table gives the total experience of some of the best known operators up to 1871 :

Operators.	Cases.	Cured.	Died.	Mortality.
Wells	440	328	112	25
Clay	250	182	68	28
Baker Brown ..	120	84	36	30
T. Keith	136	111	25	18
Bryant	28	17	11	40
T. Smith	20	16	4	20

There are two names that will always be associated with the first really good statistical results in connection with a long series of ovariotomies : the first of these names is that of Baker Brown, of London, and the second is that of Thomas Keith, of Edinburgh.

Baker Brown, from 1851 to 1864, tried hard in many ways to master ovarian cysts, but his operations were comparative failures until he adopted the treatment of the pedicle by the *cautery*. Keith says that once Brown adopted the cautery his success was immediate, and he lost only four of his last forty operations.

This, then, was the first step towards the reduction of the

25 to 30 per cent. mortality which, up to this time, had been looked upon as inevitable.

Thomas Keith reintroduced ovariotomy into Scotland. Between Lizars' single success, in 1825, up to the year 1863, there had been but one successful case.

In 1863, when Tait was a student at Edinburgh, Thomas Keith of that town performed his first operation, and up to 1872 he performed one hundred and thirty-six operations, and had attained the highest success yet achieved in Europe: 81·6 per cent. of recoveries, a mortality of a little over 18 per cent.

Keith obtained success in the following way. Baker Brown died in 1868, and his method of treating the pedicle by the cautery was not used by any other surgeon to any extent until it was reintroduced by Keith, who says: [1]

> Some years afterwards, unable to get my mortality much under one in five, for I was then ignorant of drainage, I took to Mr. Brown's method in a sort of despair. For a time it was used irregularly, and only in the worst cases, or in those not favourable for the clamp. The result of the first fifty cautery cases, published in the *Lancet*,[2] gave a mortality of less than one in twelve—8 per cent.—and the results that followed were much better. Mr. Wells and Mr. Thornton have lately given their statistics of cases performed under careful antiseptic treatment, and with all the other improvements of these later years; and the mortality is nearly 11 per cent.

Keith, under Lister's influence, was perhaps the first surgeon who attempted to carry out the antiseptic principle in the removal of abdominal tumours. Lister had showed him,[3] as early as 1868, a large blood-clot organised in the wound of a compound fracture, and after that Keith used Lister's antiseptic precautions in his daily surgical work. For nearly three years, from 1869 to 1871, Keith had the hope that a certain amount of carbolic acid introduced into the abdominal cavity might prevent or retard the putrefaction of red serum that is apt to stagnate in the pelvis after ovariotomy, and, accordingly, a two or three per cent.

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[1] *Brit. Med. Jour.*, July 31, 1880.
[2] The *Lancet*, April 15, 1876.
[3] *Brit. Med. Jour.*, October 19, 1878.

watery solution of carbolic acid was freely used in sponging and cleaning out the cavity; antiseptic ligatures, first of the finest silk and then of catgut, were used, and all instruments were rubbed with carbolic acid. Towels were soaked in the watery solution. . . . Carbolic acid was wasted in every possible way. The floor and walls of the room in which the operation was to be performed were sprayed with it, and the air was charged with the vapour driven off by the heat. For nearly three years—from 1869 to 1871—this practice was more or less carried on. The results, which for that time had hitherto been good, did not improve, but rather the reverse, and after two or three mysterious looking deaths, in cases that should have done well, Keith did not go on with the antiseptic technique.

It is an interesting fact to note that Lister himself, at this period, was not keen on Keith performing ovariotomy with antiseptic precautions. In addressing the Surgical Section at the International Medical Congress in 1881, Lister said:

> The success that has attended antiseptic ovariotomy has been regarded as a signal proof of the truth of the antiseptic principle. Such, however, has never been my own view. Mr. Spencer Wells and Dr. Thomas Keith obtained results which astonished the world before strict antiseptic treatment was thought of; and when, several years ago, Dr. Keith expressed to me an intention of performing ovariotomy antiseptically, I strongly dissuaded him from this purpose. I knew his brilliant success; I felt that the spray apparatus was as yet inadequate for the production of a cloud sufficiently large to cover the whole field of operations, and sufficiently fine to avoid needless irritation, and I was also aware that such operations are often both very protracted and very anxious, while in proportion to the duration and the anxiety of an operation is the chance of neglect of some apparently trivial, yet important element in the procedure. And if the antiseptic treatment were attempted in ovariotomy and failed in its immediate object, I felt that it would not only be nugatory but injurious. . . .
>
> At the same time, I believed that the day would come when strict antiseptic treatment would be for me valuable in ovariotomy. Especially did I anticipate that it would permit early operations for small tumours, instead of the patient being kept waiting in anxiety, as used to be the case, till the tumour should have attained large dimensions.

As the carbolic acid treatment had brought nothing but "disappointment and vexation of spirit" to Keith he abandoned it. He says:

> For the next five years, though I continued to use it as usual in ordinary work, no carbolic acid whatever, nor any antiseptic, was made use of in my ovarian operations. Unable, though believing in it as much as ever, to carry out the antiseptic principle, and thus find protection for external agencies, I thought this operation one in which it would be better to trust to care and cleanliness alone.

Lister notices this and adds a footnote to his paper as follows:

> It is important to bear in mind that Dr. Keith always paid the most scrupulous attention to cleanliness; and among the elements of that cleanliness he included what was a very important antiseptic precaution—viz. the purification of the sponges which he used, by boiling them; impure sponges being undoubtedly a very fertile source of septic contamination, while protracted boiling is a most effective means of destroying septic ferments.

During the years 1871 to 1877, when Keith was operating without antiseptics, the surgeons on the Continent, having adopted Listerism with enthusiasm, now began to apply its principles to ovariotomy, after learning of the excellent results obtained in general surgical practice by Volkmann of Halle, and Nussbaum of Munich.

Up to this period ovariotomy, though it had frequently been performed on the Continent, had always been attended with an appalling mortality. The first operation in France had been performed by a country surgeon named Woyerkowsky, at Quingez, in 1844, with success. The second operation was performed, in 1847, by Vaullégeard, of Condé-sur-Noireau, and this was successful. In 1861 Nélaton visited England to see Spencer Wells and Baker Brown operate; he returned and delivered a clinical lecture, and then, for the first time, as Pozzi remarks, the justifiability and legitimacy of the operation were proclaimed by a professor in a French university.

In 1862, in the presence of Nélaton and Trousseau, Demarquay operated in Paris on a young woman with an

ovarian tumour, but this operation was fatal. A few months later, however, Koeberlé, at Strasburg (then a French city), removed the first ovarian cyst and successfully began a series of brilliant operations which placed him in the front rank of French ovariotomists. Then came his great rival Péan; they both invented hæmostatic forceps to help the technique, each claiming that the other had copied his invention. At first for Koeberlé, as well as for Péan, there could be no question of anything but an extra-peritoneal treatment of the pedicle, and a clamp modified from Hutchinson was used for hæmostasis; later on the écraseur was used and the metallic serre-nœuds (the introducer of these latter instruments was Koeberlé); later the pedicle was dropped back, and when antiseptics were introduced, Terrier, by observing thoroughly in ovariotomy not only scrupulous cleanliness, but also the strongest rules of antisepsis and of asepsis, caused abdominal surgery to take, in France, a definite stride in the direction of increased security.[1]

In Germany, two years after the publication, in 1817, of McDowell's first three cases of ovariotomy, this operation was performed by Chrysmar of Isny, Würtemberg. The first operation was unsuccessful, but the second case, operated on in 1820, was successful.

Previous to 1850, ovariotomy had been performed twenty-three times in Germany, with seven recoveries; while from 1850 to 1863 it was only successfully performed in that country on eight occasions; the unsuccessful cases numbered twenty-four.

From 1864 to 1870 there were one hundred and twenty-five operations with sixty-five deaths.

Thus we see that, at the period when Keith was operating with marked success, the German surgeons had the appalling mortality of 52 per cent. In 1877 Olshausen collected 613 cases, with 43 per cent. of deaths.

As the continental surgeons performed more operations and adopted antiseptic technique their results improved, so

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[1] Pozzi, "The Evolution of Ovariotomy in France," *Surg. Gyn. and Obst.*, vol. lx., 1909, p. 424.

that Olshausen was able, in 1878, to write to Spencer Wells and say that the result of ovariotomy performed by the best-known surgeons had much improved since the adoption of the antiseptic technique ; and he gave the results of Esmarch, Hegar, Schroeder, and himself with and without antiseptics. *Without antiseptics* there were sixty-five cases and thirty-three deaths ; while *with antiseptics* there were one hundred and fifty-five cases with only thirty-three deaths. These results continued to improve, so that Schroeder, of Berlin, in 1884, when publishing his own results, was able to record 514 ovariotomies with 65 deaths ; a mortality of 13 per cent.

Nussbaum, of Munich, in 1884 had over 415 cases with 89 deaths ; the last 115 cases with 10 deaths.

Olshausen, in 1885, had had 293 cases with 266 recoveries ; four deaths in the last hundred cases.

The results obtained by the German surgeons convinced English operators that ovariotomy could be safely performed with success with antiseptic precautions. This was the opinion expressed by Knowsley Thornton in 1878, and he remarks that " he himself was the first to introduce antiseptic ovariotomy at the Samaritan Hospital, London, in October, 1877, only a few months later than Dr. Keith's first antiseptic case in Edinburgh." [1] The last sentence refers to the fact that after Keith had worked on without antiseptics from 1871 to 1876, he determined " to perform all my ovariotomies with all Mr. Lister's care, under the carbolic acid cloud, and I shall never go back to the old way."

Keith has asked the question, " What have we gained by antiseptics in ovariotomy ? " And he has answered in the following way :

(1) It has lessened the mortality ;

(2) The increased safety will encourage medical men to recommend earlier operations ;

(3) With antiseptic ovariotomy the drainage tube will not be nearly as often required, for convalescence is rendered easier.

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[1] *Brit. Med. Jour.*, November 2, 1878.

Keith's wonderful results, and his strong advocacy of antiseptic precautions had a very marked result, and no doubt had considerable weight with Spencer Wells, who was slow to adopt the antiseptic technique, although his colleague Knowsley Thornton had quickly become one of its most powerful advocates.

Keith's paper was published in 1878. This same year Tait completed his first series of fifty ovariotomies with nineteen deaths. Tait had employed the carbolic spray, and had followed out the precautions laid down by Lister. Something was wrong. He did not hesitate to announce that he doubted the efficiency of the Listerian precautions, but as well as doubting these precautions he struck at the weak point in his own technique, viz. the extra-peritoneal treatment of the pedicle by the Wells' clamp. In a letter written in 1886, Tait says :

> The moment I recognised the fact that the intra-peritoneal method was the one that ought never to have been given up, a recognition which I owe entirely to Keith, the moment I discovered what eminent service Baker Brown had done to abdominal surgery, then my views about Sir Spencer Wells necessarily changed. As I have said, I do not regret the change. What I regret is, that I ever entertained the views that I published in 1877; for if I had not been led astray by Sir Spencer Wells' practice, in my own experience I should have been able to save a large number of lives which otherwise were sacrificed.

From this time on Tait abandoned the Listerism technique and followed his own aseptic technique with excellent results, and it was for this reason that he continued to maintain that antiseptics had no bearing on his success. He had learnt how to be clean, Lister had taught him what to avoid, but he refused to acknowledge that Lister was his real teacher and his master.

XXVI

Opposition to the Operation of Removal of the Appendages

1886

The Ingleby Lectures were established in connection with Queen's College, Birmingham, in 1876, and were intended to promote the advancement of obstetric medicine and surgery, including diseases of women and children, being a branch of medical science in which Dr. John Tomlinson Ingleby acquired considerable reputation.

Dr. Ingleby practised in Birmingham from the year 1816 until his death, at the end of January, 1845. He was the first specialist in obstetric medicine in Birmingham, and lectured on midwifery when the medical school was first formed there in 1828.[1]

On September 3, Tait delivered the Ingleby Lecture at Queen's College, Birmingham. He chose for his subject "Pelvic Hæmatocele," and his first words were: "A trial which has recently taken place in Liverpool has given a special interest to the subject of hæmatocele, and that alone would justify me in taking up a considerable part of your time in discussing this extremely interesting subject."

The trial that Tait alluded to was Casey *v.* Imlach, and was tried at Liverpool before Mr. Justice Cave and a special jury. The action was brought against Imlach to recover damages for improperly performing an operation on the patient's ovaries and uterine appendages. Imlach said that he performed the operation for hæmatocele and hæmatosalpinx. Some of the evidence, given by the medical men called for the plaintiff, shows very well the feeling that existed against removal of the ovaries and the Fallopian tubes at that period.

[1] Wilson, "Pelvic Inflammation in the Female," Bristol, 1907.

Dr. Wallace, at that period professor of midwifery at the Liverpool University College, in giving evidence, said: "He would not perform excision of the ovaries except in very extreme cases, and after very long observation. . . . It was a well-known fact that it produced masculine appearance. . . . If there were salpingitis and adhesions to the Fallopian tubes and ovaries, that would not justify the operation."

Mr. Steel, surgeon at the Women's Hospital, Liverpool, said that if the patient were suffering from hæmatocele and hæmatosalpinx "he did not think there would be sufficient disease to justify the excision of the ovaries."

All the medical witnesses were questioned as to the effect of the operation on sexual desire.

After Imlach had given evidence, Tait was called. The following is from the report of the trial in the *Lancet*: [1]

> Lawson Tait, F.R.C.S., and surgeon to the Birmingham Hospital for Women, said he performed the first operation for the removal of uterine appendages in February 11, 1872, and, including abdominal sections, he had operated about 1600 times. Intra-peritoneal hæmatocele was very distinct from extra-peritoneal and was fatal in 96 per cent. of the cases if left long. In the great majority of cases it was fatal in three hours, and the longest time he has known before it was fatal in one attack was forty-eight hours. He has heard the evidence of Dr. Imlach, and from his description of the case he should say that the operation was a most proper one.
>
> Mr. Bingham.—You have heard his diagnosis before the operation, what does that indicate as to the sterility of the patient?—There can be no doubt that the woman was completely sterile before the operation. His account of the operation itself confirms the diagnosis.
>
> From your experience what effect has this operation upon the sexual desire?—In no case that I have ever asked the question have I found it destroy the sexual appetite at all, and in very many cases, where the disease destroyed it, the operation has restored it. Chronic salpingitis with adhesions apart from or connected with hæmatocele would be a proper case for operation if the symptoms were severe enough. From the plaintiff's own description of the symptoms, I should say that is a case of recurring intra-peritoneal hæmatocele. She described the case exactly.

[1] The *Lancet*, August 14, 1886.

His Lordship [Mr. Justice Cave]: You say the longest period before death you have known has been forty-eight hours; but here you have a woman who has three previous attacks, none of them fatal?—I know no case like this; it is unique.

Mr. Bingham.—From the diagnosis you have heard of December 8 and 17, would there have been any danger in leaving the case longer?—I say Yes; because the amount of blood found at the time of the operation was very considerable, and if the bleeding had been going on the patient would certainly have died. It most certainly was an urgent case according to the amount of blood.

His Lordship: In the case of intra-peritoneal effusion how is the blood got rid of?—A great majority die of the hæmorrhage.

Cross-examined by Mr. French.—Before the action was brought was there a discussion amongst medical men on this question in which you took part? Was there a printed discussion between you and Liverpool doctors with regard to Dr. Imlach and his system of treatment at this hospital in Shaw Street?—There were several letters published in medical journals.

There is a letter of yours in the *Medical Press* of March 3, and I will read to you a passage: "I assumed, of course, in my previous discussion of this matter in your columns that Dr. Imlach was actuated by principles similar to those which I myself had adopted. I was not yet prepared to say he is actuated by others, but I am bound to say that after reading the discussion in your columns to-day, and still more after reading the paper by Dr. Imlach himself in the *British Medical Journal* of last Saturday, I think he has departed from those principles. He certainly has gone a great deal further than I think can be justified in the removal of appendages for hæmatocele; but I want to guard against condemning Dr. Imlach, because, after all, it may be proved that he is on the right side of the question. At present I do not think so." Can you now point out anything in the course of the case to make you change the opinion you expressed in this letter?—The opinion expressed there is very simply explained. It was from a want of precision in Dr. Imlach's description of the two kinds of hæmatocele, and I assumed, perhaps too hastily, that he removed the appendages for extra-peritoneal hæmatocele. I afterwards found that this assumption was not correct. In intra-peritoneal hæmatocele surgical operation is absolutely demanded, and immediately.

Can you explain why Dr. Grimsdale has not had occasion to remove the ovaries?—I can point to a large number of cases which have passed through Dr. Grimsdale's hands,

which have come to me for operation, and the explanation is that he has not followed the advances made so as to recognise the value of this operation.

Among other surgeons who gave evidence for the defence were Aveling, Savage of Birmingham, also Greig Smith of Bristol. At the conclusion of the evidences the jury intimated that they had made up their minds about the case and returned a verdict for the defendant.

No sooner was this case over than there were discussions and letters in all the medical journals on the propriety of removal of the Fallopian tubes and the ovaries.

The *Lancet* in a leading article said :

> The interest of the trial lies, however, far less on the narrow issue which the jury had to deal with, than on the broad question whether the frequent and almost indiscriminate spaying of women suffering from diseases which are not fatal, and which are often only trivial, is justifiable. . . . The highest authorities in this country have justly condemned the frequent performance of spaying, and it is well known that such practice is limited to one or two towns in the kingdom. In America the operation has been performed with great frequency, but the pathologists and physicians have begun to question the frequency of diseases of the uterine appendages calling for their removal; and they assert that ovaries are removed and called cystic, which are in reality typically healthy organs. That there are certain cases in which the operation is justifiable and called for cannot be denied; but the idea that such cases are common is entirely erroneous.[1]

After a few weeks the *Lancet* again returned to the attack in a sub-leader :

> Diseases of the uterine appendages have been brought during the last ten years into great prominence—a prominence which has not been shown to be deserved. This state of things has been brought about, not by any careful study of the physiology of the organs involved, or of the etiology and pathology of the diseases to which they are subject, but by the frequency with which women have been made the subject of the operation of spaying—in fact, by the clumsy method of "setting the cart before the horse." The great frequency with which the uterine appendages are the seat of disease demanding their removal has often been asserted,

[1] The *Lancet*, August 14, 1886, p. 304.

but on evidence which has quite failed to carry conviction to the minds of the profession in this country; where the beneficial results of the operation of spaying in such cases, although asserted and reasserted, have not as yet received such evidence in their support as to place the operation in the position in which those who frequently have recourse to it would fain see it.

Not only did the trial at Liverpool give rise to considerable controversy on the subject of the removal of the appendages, but some medical journals took a delight in alluding to the operation as "spaying." Tait always had a great objection to the term, and he wrote to the *Lancet*, and objected to this term being used. He says: [1]

I am greatly surprised to find in the classic columns of the *Lancet*, a continuation of that extraordinary confusion of terms between "spaying" an animal and "removal of the human uterine appendages." Animals are spayed before puberty to prevent its access, and the consequent development of a sexual appetite and powers, and the powers of procreation. The ultimate object of the process of spaying is an improvement in our food supply and in our domestic service of animals.

Removal of the human uterine appendages is performed after puberty—generally long after puberty. It has no influence, so far as I know, in destroying the sexual powers, indeed, it constantly restores them after disease has destroyed them. It does not usually destroy the procreative power, for that (in the great majority of instances) has been already destroyed by the disease for which the operation has to be performed. Finally, the operation has no other purpose than the saving of life and the relief of suffering. The operation of spaying an animal and the removal of the human uterine appendages have therefore no conceivable resemblance. Consequently to talk of "spaying women" is to display great ignorance or to indulge in wilful misrepresentation for purpose of giving offence.

The *Lancet*,[2] in a sub-leader, replies to the above:

The term "spaying" is one whose meaning is well understood generally by medical as well as non-medical persons. As practised in this country upon animals, the operation consists in the removal of the ovaries and Fallopian tubes, or uterine appendages. Objections have been raised to the

[1] The *Lancet*, August 21, 1886, p. 375.
[2] The *Lancet*, September 4, 1886, p. 453.

term on grounds which are untenable. An operation should be named not by reason of the motive with which, or the purpose for which, it is done, but according to its nature from an anatomical point of view. In England spaying means the removal of uterine appendages, and castration the removal of the testicles, with whatever motive or for whatever purpose such organs are excised. In Germany there is but one term "castration," for the two operations, and "Castration der Frauen" is the phrase used for the removal of the uterine appendages in the human female, and is the equivalent of the English phrase "spaying of women." When Battey proposed the removal of one ovary in the human female for certain suffering, he suggested the term "normal ovariotomy" for the operation, because it consisted in the removal of an ovary still functionally active. That is not spaying, and the word "spaying" would have been inapplicable to it, for the word means removal not of one but of both ovaries. It was, however, soon discovered that the removal of one ovary was insufficient, and that if operations for the removal of active ovaries were to be performed at all, both glands should be removed in every subject. Now the term "spaying" became appropriate and the only appropriate English word for the operation, and intelligible to everybody. At this juncture Dr. Marion Sims proposed the expression "Battey's operation," and it was adopted, but in a half-hearted sort of way. Mr. Tait calls the operation "removal of the uterine appendages," a phrase to which there can be no objection on anatomical grounds, but which is clumsy when compared with the single and expressive word "spaying," and has at the same time the disadvantage of being quite unintelligible to all except scientific persons. Some Americans have called it "Tait's operation." Three surgeons claim the discovery of this innovation: Battey in America, Tait in Birmingham, and Hegar in Germany. The operation has been called after the first two by some, both in England and in America, but no such folly has been perpetrated in Germany. There the term "castration" is applied to the operation, and no patient can mistake what it means; understood of all—a word which was used for the operation before Battey, Tait, or Hegar thought of performing it in the human female—with such a word for our use, it would be ridiculous and absurd to use a long phrase to designate it. We can conceive no objection to the word "spaying," but the patient upon whom the operation is to be performed should clearly understand what she is really about to undergo.

In spite of all Tait's efforts, it took years to convince some members of the profession that the removal of the uterine

appendages was a right and proper operation, but he did so in the end. Gradually men who opposed his views, whether from prejudice or from want of experience, were convinced because of the overwhelming evidence gradually brought forward on all sides. During 1886 Tait read a paper before the Medical Society of London on "The General Principles of Removal of the Uterine Appendages," and he had the gratification of hearing Knowsley Thornton say that he was prepared to agree with the greater part of the paper.[1] This meant that one of the foremost gynæcologists in London had acknowledged Tait's main contentions about the justification of removal of the appendages. This could not fail to have a very decided influence on many who practised in the metropolis, and who regarded Tait's operations with distrust and contempt; chief amongst those being Sir Spencer Wells, and it was in reply to a paper by Wells in the *International Journal of Medical Sciences* that Tait had read his paper at the Medical Society.

The articles that have been contributed to the *Lancet* had been written, or inspired by Spencer Wells, and Tait further maintained that Wells had provided the funds for the prosecution of Imlach. He alludes to this in his address to the British Gynæcological Society when he says: "We want to know who provided the funds for the prosecution of Dr. Imlach, and who are the anonymous writers in two medical papers who have so unfairly and unjustifiably misrepresented the questions at issue, and the grounds upon which they have been decided."

In the large provincial towns Tait had a much greater following, and Cullingworth, at this time Professor of Gynæcology at Owen College, Manchester, read a paper[2] on the removal of tumours of the ovaries and adjacent parts, and he said at the outset that while the operation for the removal of the ordinary ovarian tumours had become

[1] In 1914, Alban Doran wrote to us his personal opinion of Tait; in one passage he says: "The great objection to Tait's operations on diseased appendages was the lack of sufficiently long after-histories, but that I could not dwell on in an obituary notice—he never seemed to take neuroses into account."

[2] *Brit. Med. Jour.*, January 30, 1886, p. 193.

general, yet "the feasibility of operative interference in the class of cases, of which the two to which I am about to call attention are typical specimens, is not yet established on anything like so firm a basis. We are, indeed, only just beginning to realise the vast fields of usefulness that have been opened up to us by recent advances in abdominal surgery—advances due, in the first instance, to the experience gained by our great ovariotomists; in the second place, to the introduction of the various antiseptic methods, whether Listerian or otherwise; and lastly, if not chiefly, to the conspicuous ability and enterprise of a distinguished provincial surgeon; I mean, of course, Mr. Lawson Tait."

In spite of the opposition of many of the London surgeons to Tait this did not prevent him being elected to the presidency of the British Gynæcological Society, and Alfred Meadows, the retiring president, spoke of Tait in the following words:

> Mr. Lawson Tait has, by his work, demonstrated that he is a consummate master of his art, and he is known to be a man of the true scientific type. Moreover, he is a man of great force of character—fearless, honest, thorough, and straightforward; in fact, just the sort of man that I would wish to see ever at the head of affairs of the British Gynæcological Society. I congratulate you on the choice which you have made, and him on the honour he has received.[1]

The founding of the British Gynæcological Society had an important influence on the minds of medical men, not only in London, but also in the provinces. The society began its work in 1885, and was composed of men who were eager to advance the study of the diseases of women, as opposed to the members of the Obstetrical Society who took a deeper interest in purely obstetrical problems. The new society soon had a goodly roll of members, and when Tait, as second president, gave his annual address in 1886, he was able to announce that there were already 460 members on the roll. Many of these men came to the monthly meetings in London, saw the specimens of diseased appendages exhibited by Tait, Savage, and others, and were at once convinced that a new era had started in pelvic surgery.

[1] *Brit. Med. Jour.*, January 30, 1886, p. 183.

The opposition that had existed among a number of old men in London, led by Spencer Wells, who was supported by Playfair, John Williams, Mathews Duncan, and others, still was as bitter as ever, for these old fellows knew that the ground was being cut from under them, yet they refused to acknowledge their defeat. Men like Knowsley Thornton and Alban Doran, thorough friends and colleagues of Spencer Wells, had the sense to see that Tait and Savage were in the right, and so they fell into line with the new thought, and that is how Thornton came to admit, at the Medical Society in 1886, that he was prepared to agree with the greater part of the views contained in the paper, read by Tait before the Society, on "The General Principle of Removal of the Uterine Appendages." When Cullingworth came to live in London, he, of course, openly espoused the cause of Tait and his followers, and soon, with Bancroft, became an invaluable ally in the fray.

Of the younger men, Greig Smith, of Bristol, who had gone to Birmingham to study Tait's methods, was a new force. He produced, in 1887, his book on "Abdominal Surgery." This book, which Tait described as a "masterly work," gave the best account of Tait's own work.

Mayo Robson, of Leeds, and Smyly, of Dublin, had both come to Tait, had remained as his pupils, and each in turn became his steadfast follower. In London, at this period, a young man published a small work which showed that he had an original bent of mind. A few years later he published another work entitled "Evolution and Disease," and at once became known. Later, he brought out his work on "Diseases of the Ovaries, Fallopian Tubes, and Ectopic Pregnancy"—that man was Bland Sutton. He resided in London, and was attached to a London hospital. He became an invaluable help to the new school, for he soon established his name as a pathologist, and later as a surgeon. Although Tait could not agree with Bland Sutton's views on tubal abortion, time has proved that Sutton was right and Tait wrong.

No great change has ever come about in any branch of science or medicine without opposition; and it is perhaps

as well that this should be so. Opposition means that reasons are propounded why a thing should not be done, and it then becomes the duty of those who wish to introduce the innovation to bring forth their reasons to justify the procedure. It is this thrashing out of the pros and cons that serves to make new procedures more perfect, if they are capable of standing the proper tests. Were we to take up every new idea and blindly adopt it because its advocates, in perfect good faith, thought that the ideas were correct, then more harm than good would be the inevitable result. There cannot be the slightest doubt that the gynæcologists of the period that we are dealing with did remove hundreds of ovaries and tubes that should not have been touched, and no one knew this better than Tait himself; and by exhibiting his specimens at the Gynæcological Society he sought to teach his less experienced surgical brethren the difference between diseased and non-diseased tubes and ovaries. Tait, in 1891, in speaking of operations on the tubes, said:

> There can be no doubt whatever that within a very short time there has been a large amount of grave malpractice upon this point, and a large number of patients have been operated upon who ought never to have been touched. A few years ago I had to pay a professional visit to a town of fair size, not very far removed from the spot where I now am, and where there is a country hospital somewhat famous for its surgical activity. I there met a gentleman for whose many virtues I have much respect, though one virtue, that of surgical enthusiasm, I think he carries to the point of error. To my great delight he professed himself as one of my ardent disciples, and he triumphantly brought out a series of some two dozen bottles containing the results of what he was pleased to call "Tait's operation." As soon as I had him by himself, I informed him in very ardent language that he must never, under any circumstances whatever, attribute to me the awful proceedings involved in the display of those dreadful bottles. They contained sets of uterine appendages, in almost every instance, free from guile, not such as I should have undertaken to remove save under circumstances so exceptional as to be hardly worth an estimation. As for their being the result of "Tait's operation," I entirely disowned the suggestion.[1]

[1] The *Lancet*, February 7, 1891, p. 296.

XXVII

Cysts of the Urachus

1887

In his annual address to the Obstetrical Society of London, in 1900, Alban Doran said that in 1887 Tait

> published a remarkable series of operations for cystic collection of fluid in the anterior and inferior part of the abdomen. Although his theory that they were allantoic or urachal was based, according to his own admission, on defective evidence, it must, on the other hand, be allowed that in reporting these cases Tait succeeded in turning the attention of surgeons to the pathology and operative treatment of cysts of the urachus.

In 1909 Doran read a paper before the Royal Society of Medicine,[1] and in that communication he makes the following statement:

> In 1898 I showed how the long series of cases reported by Lawson Tait had been misinterpreted by that great surgeon when he ranked them as urachal cysts, excepting one published by himself and Teichelmann in the *Lancet* twenty years ago. Several writers who have apparently never read my criticisms have recently expressed precisely the same views. Meriel, Binnie, and Delore and Cotte all maintain, as I did, that Tait's series were instances of encysted peritonitis, probably tuberculous, where, as I pointed out, communication between the bladder and peritoneal cavity is not infrequent. I may add that Mr. Tait himself, in a letter about my monograph in 1898, informed me that he had detected mature hair-follicles in sections of the cyst-wall in one case. This would imply that the cyst was a universal adherent ovarian dermoid, or a dermoid tumour derived from some other organ, but urachal it could hardly be.
>
> There were no after-histories to these cases, and no verification of the relations of the cysts at necropsies. Hoffmann's case, where the cyst contained fifty litres of fluid when opened at the post-mortem, was shown by Wutz to be an

[1] "Urachal Cyst simulating Appendicular Abscess: Arrested Development of Genital Tract: with Notes on Recently Reported Cases of Urachal Cysts," Surgical Section, March 9, 1909.

example of *chronic hæmorrhagic peritonitis*. The original report was so defective that an absurd error about the sex of the patient was included. Yet Delore and Cotte agree with Hoffmann, though rejecting Tait; and in articles on urachal cysts by living writers Tait's series, and Hoffmann's equally spurious case, are persistently included as though they really represented urachal disease.

It will thus be seen that while Tait is credited with creating the surgery of urachal cysts, he yet is deprived of the honour of having operated on any such cyst except the one reported by him and Teichelmann in 1888.

In order to understand the position we must go fully into the matter.

Cysts of the urachus were known before Tait published his first cases.

As far back as 1530 Cabral studied a vesico-umbilical fistula in a young girl; he made a correct diagnosis; established its pathology, and suggested a form of treatment.

When ovariotomy first began to be practised urachal cysts were met with from time to time. Thus Washington Atlee related that when incising the abdominal wall, in the midline, that he cut into a small cyst before he penetrated the peritoneum. Fluid was discharged for days after the operation. He remarks, "the only conclusion possible, considering the extra-peritoneal and elevated locality of this urinous cyst, is that it was a purse in a dilated urachus, which, although closed at the umbilicus, had from birth maintained a communication with the bladder." [1]

Spencer Wells, in 1885, wrote: "In one case of ovariotomy I found the urachus, though closed at both ends, open for the whole length of my incision in the abdominal wall, and filled by small urinary concretions." [2]

Even before these surgeons recorded such instances of patent urachus, Luschka, in 1860, said, "I have no doubt of the occurrence of large cysts requiring surgical interference, and that these cystic swellings found in the abdominal wall have their origin in the development of the urachus."

The report of Hoffmann's two cases in 1870 seemed to

[1] "Ovarian Tumours," 1873, p. 50.
[2] "Surgical Treatment of Abdominal Tumours," p. 84.

establish the fact that cyst of the urachus was a definite pathological condition amenable to surgical relief; but Hoffmann's cases are now classed with Tait's, and are not accepted as urachal.

Wolff, in 1873, gave full details of a case on which he had operated.

Tait's first cases were published in the 1883 edition of his "Diseases of the Ovaries;" but his communication in 1887 to the British Gynæcological Society on "Twelve Cases of Extra-Peritoneal Cysts" first fixed the attention of surgeons on the operative treatment of cysts in this region of the abdominal wall.

Tait's paper was followed by cases reported in America by Byron Robinson, Freer, Ill, Fergusson, and others.

In order to understand the origin of urachal cysts we must study the subject through embryology.

The urinary bladder with the urachus is the persistent portion of the allantois, and Hertwig states that the segment of the allantois which is included in the body of the fœtus appears in the human embryo of the second month as a hollow fusiform body.

In its middle part a dilatation appears which in time becomes the bladder; while its upper part becomes the urachus, a tube lined with epithelium.

After birth atrophy proceeds in an irregular manner, and Jahn states that in time the connective tissue presses at different points against the urachus so as to cause septa to develop in its canal; and so we may have developed a row of bead-like cysts, as pointed out by Luschka and Wutz.

A valvular fold of mucous membrane covers the vesicular orifice of the urachal canal and prevents the escape of urine from the bladder; this is called THE VALVE OF WUTZ.

Binnie [1] in a paper on the "Development of the Urachus," says:

> Early in their development the urinary bladder and the urachus are completely surrounded by peritoneum, except on the ventral surface where a mesocyst [mesentery of the

[1] *Jour. Amer. Med. Assoc.*, 1906, xlvii. p. 100.

bladder] exists. This disposition usually disappears and the urachus becomes extra-peritoneal. That the mesocyst sometimes persists was well exemplified in an adult cadaver examined for me, in which the urachus did not lie close to the abdominal wall, but lay between loops of small intestine. The persistence of the mesocyst explains the occurrence of some otherwise puzzling intra-abdominal cysts.

We find, then, that while the urachus is usually adherent to the belly-wall and is extra-peritoneal, it may sometimes be found not close to the parietes, but may lie between folds of small intestines. Doran therefore says :

"Thus, on the testimony and experience of independent embryologists, anatomists, and surgeons, we learn that the urachus may possess a mesentery, or at least a fold of peritoneum, representing most probably the persistence of an embryonic condition.

With regard to the urachal canal the sheath is composed of white fibrous tissue towards the umbilicus ; while towards the lower or bladder end we find chiefly plain muscle fibres. The canal lies between the transversalis fascia and the peritoneum, and is surrounded by loose connective tissue.

Coming now to cysts of the urachus, we find they are grouped under two headings. The first kind is a cystic dilation of the urachal canal, which opens into the bladder, but is closed at the umbilicus ; while the second variety is the true cyst of the urachus, which may be unilocular or bilocular. Doran operated on a case where the cyst was bilocular, the larger and upper cavity containing over a pint, the smaller about half a pint, of greenish-brown fluid. These true cysts of the urachus are usually not much larger than a cherry, and sometimes several small dilatations are formed causing the urachus to assume a moniliform appearance. In other cases the canal of the urachus is slowly dilated and may hold a pint, a quart, or even more. The structure of these large cysts is identical with that of the bladder, and consists of unstriped muscle fibre lined on the inner side with epithelium, similar to that found in the vesical mucous membrane ; but in many instances the urachal epithelium is destroyed. The cyst is sometimes connected with the bladder by a thick cord, an inch or less in length, and when sections are cut of this segment

of the urachus it is found to be lined by transitional epithelium, and the muscular coat may be of considerable thickness.

When we come to examine the reported cases of urachal cysts we find that a cyst may be monolocular or bilocular; it may be sessile on the bladder, or it may be connected with the bladder by a pedicle formed of portion of the urachus. In some cases the cyst is very small, while in other cases the cyst is reported to have held up to eighteen pints of fluid.

In the great majority of cases these cysts are extra-peritoneal, but in rare cases the cyst may be provided with a mesentery, as related by Delore and Cotte, and it thus becomes an intra-peritoneal cyst as it develops. When a urachal cyst is intra-peritoneal, the urachus most probably had a mesentery before the cyst existed (Doran). One would expect that cysts of the urachus would lie in the mid-line, but this is not always the case. A cyst, for instance, may lie in the right iliac fossa, the middle line forming its limits to the left; while, on the other hand, one of the urachal cysts reported by Weiser filled the whole left side of the abdomen.

We will now give a brief description of some cases that appear to be beyond doubt, as regards the urachal origin of the cyst, and then we will proceed to consider Tait's cases.

Doran operated on a case in 1908. She was 17½ years of age, single, and complained of tenderness and swelling in the right iliac fossa. There was a fairly well-defined, almost spherical swelling in the right iliac fossa, slightly movable and tender to touch. At the operation an incision was made in the middle line. After separating the recti a thick membrane was encountered, and lower down the wall of the bladder was exposed extending two inches above the pubes. The membrane was cut through, and about half a pint of clear fluid removed; the fluid lay in a cyst behind the recti, and anterior to the parietal peritoneum; the membrane mentioned above proved to be the anterior wall of the cyst. The cyst was connected with the bladder by a thick cord half an inch in length. The upper limits of the cyst lay just below the umbilicus.

Sections of the cord-like structure, which ran on the surface of the parietal peritoneum between the fundus of the bladder and the cyst, were examined. The canal was patulous and lined with perfect transitional epithelium of the bladder type. The lumen was free from catarrhal or other morbid products. The muscular coat was abnormally thick. Its inner portion was mostly made up of circular, and its outer portion of longitudinal, fibres.

The examination showed that the urachal canal must have been closed at the bladder end, or if not, there must have been an efficient Wutz's valve, as the contents of the cyst showed no evidence of being fouled by urine.

Morestin operated on a woman aged twenty-four suffering from a suppurating Fallopian tube. When making the abdominal incision he found two cysts, one lying above the other in the middle line between the recti, and in front of the peritoneum. The urachus could be seen running from the fundus of the bladder into the lower cyst, and from the upper cyst to the umbilicus. The cysts did not adhere to the peritoneum behind them. After removal the cysts were examined: the outer wall consisted of connective tissue, the inner was lined with pavement epithelium.

The above are examples of cysts clearly connected with the bladder and placed between the peritoneum and the recti muscles. The following is an example of an intra-peritoneal cyst. The case is recorded by Delore and Cotte. The patient was a girl, aged twenty, believed to be the subject of tuberculous peritonitis of the ascitic type. There was uniform distension with distinct fluctuation. A large cyst was exposed when the parietal peritoneum was excised. It contained blood-stained fluid, and when emptied and drawn through the abdominal incision, Delore found that it was connected below with the fundus of the bladder by a short cord, which formed an entirely extra-peritoneal pedicle and was clearly the lowest portion of the urachus. When the cord was severed, the cyst was found free, and on examination the outer wall of the cyst was the muscular coat of the urachus, while the inner wall had been greatly altered and deprived of its epithelium by inflammatory

changes. The cyst was invested over the whole of its extent by peritoneum, but at the level of the attachment of the cord connecting it with the bladder, the fold of serous membrane ceased.

We will now glance at the twelve cases reported by Tait.

1st Case.—In this case the cyst wall appeared after division of all the layers except the peritoneum. Thirty pints of brown thick fluid, with an abundant flaky yellow deposit, consisting chiefly of pus and mixed with large fibrous masses, were removed by tapping. The cyst wall was completely enucleated without entering the peritoneum. The intestines and the pelvic organs could be felt through the anterior peritoneal fold, non-adherent, and so far as could be determined, were perfectly healthy. The inner surface of the cyst consisted of broken-down epithelium, infiltrated everywhere with pus lying upon the basement membrane, which consisted almost entirely of muscular fibres. The patient died of exhaustion three days after the operation.

2nd Case.—The cyst was tapped and seven pints of putrid urine drawn off. It was everywhere adherent, but was separated as far as possible behind. The wound was closed and drained with tube. A urinary fistula remained about a month after the operation; the patient aborted and died.

3rd Case.—Cyst gangrenous and full of pus. Peritoneal cavity not opened. When stitches removed, reopened and discharged dark unhealthy pus. Patient died on seventeenth day.

4th Case.—The peritoneum covering the cyst left the abdominal wall about two and a half inches above the umbilicus, and presented a curvilinear fold running down, outward, and backwards, symmetrically on each side, to about the middle of the crest of the ilium, and this was reflected at once on to the promontory of the sacrum. Behind this apron, consisting of the cyst-wall and peritoneum united, the intestine and other organs could be felt. The whole of the pelvis was entirely destitute of peritoneum, and was occupied instead by a cyst-wall; and standing up in the middle of the cavity, bounded on both sides by the cyst-wall, was the uterus, and what ought to have been the broad

ligaments. The bladder had a similar relation to the cyst-wall that it should have had to the peritoneum; and then, from the base of the bladder, running up and lining the surface of the transversalis fascia, was the continuation of the cyst-wall. The fluid of the cyst was clear, and floating in it were shreds of delicate membrane, with lumps of fat in it, presenting entirely the appearance of the omentum of the fœtus. The cyst was emptied, sponged out, and drained. Suppuration set in, and killed the patient six weeks after operation.

5th Case.—Condition found like the 4th case save that the contents of the cyst were purulent. Drainage and death.

6th Case.—Cyst emptied and drained. Case did well for twelve days when all the urine began to appear through the tube. Sinus finally closed.

7th Case.—Cysts evacuated by incision; six quarts of clear fluid taken away. Drainage. Cured.

8th Case.—Condition same as case 7. Cured.

9th Case.—An intra-peritoneal cyst found containing nine pints of fluid. Peritoneum not opened, drainage by tube being passed through the retro-uterine cul-de-sac into the vagina. Cured.

10th Case.—Had been previously operated on by another surgeon. Case found inoperable. Drained. Cured.

11th and 12th Cases.—These cases were not unlike the others. Portions of the cyst wall of case eleven were removed and examined by Bland Sutton.

These are the twelve cases which Tait reported to the British Gynæcological Society in 1886, and they at once directed the attention of abdominal surgeons to such cysts, and we find that in 1906 Weiser [1] was able to collect eighty-six cases of cysts of the urachus, some of which have, however, been rejected by more recent writers; the rejected cases include those reported by Tait, Robinson, and Hoffmann.

As early as 1883, Wutz, who had made an elaborate study of urachal cysts, denied that any large clinically-important cyst of the urachus had been reported up to that time. In

[1] "Cysts of the Urachus," *Annals of Surgery*, October, 1906, p. 529.

1898 Alban Doran read a paper before the Royal Medical and Chirurgical Society on a case of "Cyst of the Urachus," and he then made the statement that Tait's cases, though of high interest and deserving of further consideration, were not certainly of allantoic origin. Later on in 1909, in a paper read at the Royal Society of Medicine, Doran again asserted that Tait's cases were not instances of urachal cysts, and we have given some quotations from Doran's paper in the beginning of this chapter. Since this writer has attacked Tait's cases all subsequent writers have followed suit, consequently Meriel, Binnie, and Delore and Cotte, all maintain that Tait's cases are instances of encysted peritonitis, probably tuberculous.

No one has any difficulty in coming to the conclusion that some of Tait's cases were *probably* tubercular, for even Byron Robinson, who reported four cases of urachal cyst on which he had seen Tait operate, remarks about one that the cyst-wall was very thick and brittle, could not be separated from its bed, and the contents were thick and pultaceous; while in another of his cases he remarks that the diagnosis was cyst of the urachus, but the cyst wall was found cheesy and friable as were also the contents; to the naked eye the condition was tuberculous.

But it would be an extraordinary thing for one surgeon to operate on sixteen extra-peritoneal cysts in the mid-line and for not one of them to be a cyst of urachal origin. This is what Doran wishes us to believe. Let us see what Bland Sutton has to say on the subject.

In the case of the eleventh cyst recorded by Tait in his series, we find that portion of the cyst-wall was excised and sent to Bland Sutton for microscopic examination. He found a mixture of fibrous and not-striped muscle tissue, arranged in fasciculi, closely corresponding to the disposition of the bundles of tissue which make up the walls of the urinary bladder. Scattered throughout the whole thickness of the sections were small calcareous nodules. It was difficult to make out any definite epithelial investment to the section; but on scraping the smooth surface of the specimen with a cover glass, the field of the miscroscope

became crowded with flattened, rounded, and pyriform cells, similar to those found lining the interior of the urinary bladder, only very much smaller; and he considered that, "as the urachus is lined with epithelium agreeing in shape, and continuous with that found in the interior of the bladder" the evidence in favour of this cyst being allantoic was complete.

We may therefore grant that at least one [1] of Tait's cases was a urachal cyst, and if there was one there is no reason to suppose that some of the others were not urachal, particularly as after the operation on the sixth case, urine began to be discharged twelve days later through the drainage tube. Doran's reply to this would be that the urine was due to a tubercular fistula into the bladder; while we say it was due to the valve of Wutz, guarding the vesical orifice, giving way, and allowing the urine to escape up from the bladder to the urachal cyst; the sinus in this case soon closed.

Whether we accept the first cases reported by Tait as true cysts of the urachus, or, as Binnie thinks, as "samples of encysted peritonitis, some of them at least being of tubercular origin," no one suggests that the case reported by Tait and Teichelman in the *Lancet* in 1888 was anything else than a urachal cyst—even Doran admits this cyst as genuine.

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[1] Alban Doran contributed an "estimate" to the *British Medical Journal* when Tait died, and he said: "In 1887 he recorded a large series of operations for cystic collections of fluid in the anterior and inferior part of the abdomen. . . . There is reason to suspect that some of these cases were simply encysted dropsies due to tuberculous disease . . . still there can be little doubt that in more than one instance the tumour was urachal."—*Brit. Med. Jour.*, June 24, 1899.

Thomas S. Cullen has collected the cases reported as urachal cysts in his book, "Diseases of the Umbilicus," Philadelphia, 1916.

XXVIII

Medical Defence Union—Ectopic Pregnancy

1888

The Medical Defence Union was founded in 1885, in London. But it was a failure because it was not founded on correct lines. In 1888 it was reconstructed in Birmingham, and Tait was elected President. He made the Defence Union a success,[1] and he continued to be the President until 1892, when, at the Church Congress held at Folkestone, he spoke about the vivisectionists in such a way that the profession deeply resented his words, and he resigned his presidency. The Medical Defence Union then moved to London. Victor Horsley was elected the new President, and he continued the work Tait had begun. The Defence Union then became the powerful organisation that it is to-day, and it stands between the medical men and those who would sue or blackmail them; it gives him the support of his fellow practitioners, and often makes those dissatisfied patients, who would like to enter into litigation, think twice before they attempt to sue for damages.

Ectopic Pregnancy

In 1888 Tait was asked to deliver the Ingleby Lecture at Birmingham, and he chose for his subject, "Ectopic Pregnancy."

As Tait's name will be for all time connected with the

[1] Tait made the *Defence Union* a success because here his splendid business ability and his power of organisation—qualities which are conspicuous by their absence in ninety per cent. of the profession—had full scope. He also had a great fondness for medico-legal disputes, and frequently wrote letters on points in medical jurisprudence.

subject of ectopic pregnancy, we propose to review his lectures on this subject at length, and to give the more recent views on some points upon which he was very emphatic. Naturally, as he was a pioneer in the field, some of his conclusions during the last thirty years have been controverted, so we propose to examine each important part of Tait's work, and then give the more modern views in order to show where Tait's original observations have been confirmed, and where experience has led to the adoption of different conclusions.

Tait begins his subject with the statement that, following Robert Barnes, he has adopted the term " Ectopic Pregnancy " in preference to " Extra-Uterine Gestation."

The term " ectopic " was first used by Barnes in 1873 ; the word being derived from ἔκτοπος (out of place) ; nevertheless we may remark that Barnes, in his work on the " Diseases of Women," published in 1873, employs the term " extra-uterine gestation " in dealing with the subject. Tait says that after much consideration he decided to use the term " ectopic pregnancy " as the best which can be applied, for it gives a convenient and very complete definition, without expressing any theoretical explanations of the condition. He points out that the cavity of the uterus is the proper place for any gestation, but a gestation may be ectopic without being *extra-uterine*, as in what has been called the *interstitial* or *tubo-uterine variety*. Clarence Webster took the same view in 1895, when he wrote his work on " Ectopic Pregnancy," and remarks that " ectopic pregnancy " includes all gestations developing outside the uterine cavity, being therefore more comprehensive than the term " extra-uterine," which in strictness cannot be held to include *interstitial* pregnancy. Ectopic gestations also include those in which the ovum is implanted in the rudimentary horn of a bicornuate uterus. Parry (1876), and Taylor (1899), also Howard Kelly (1906), and Whitridge Williams (1912), use the term " extra-uterine pregnancy," while Bland Sutton and others prefer the term " tubal pregnancy."

Tait points out that the literature of the subject is very

extensive, and the confusion which existed at the period when he wrote was almost as great. He, however, acknowledges two works of importance—the first was compiled by Dr. William Campbell, a teacher of midwifery in Edinburgh, who published, in 1842, a work in which all the known published cases were brought together. The author, however, did not know how to deal with his material and the result is a collection, confusing alike in its abundance and its utter want of arrangement. "His work," Tait remarks, "however, stands as a landmark in the literature of the subject as the first real effort to place into its appropriate position of importance a subject which, up to that time, seems to have been regarded more as a curiosity than as one of the most dreadful calamities to which women can be subjected."

Bland Sutton says, of Campbell's book, that it is useful, as it reveals the slender and unreliable character of the evidence on which the varieties of extra-uterine gestation were based in the early part of this century.

John Parry, of Philadelphia, published, in 1876, the first notable book on the subject.[1] Tait says of this work that it is at once remarkable for its scholarly research and fine critical sagacity; he acknowledges how useful the book had been to him. At the present day we may trace the history of the development of our knowledge of ectopic pregnancy in medicine and surgery in the works of Campbell, Hecker, Parry, Tait, Bland Sutton, Werth, Webster, and Taylor. Prior to Parry's work, extra-uterine pregnancy was considered a rare condition; but after Tait had operated on his first tubal rupture in 1883, and pelvic operations became more frequent, this condition was then recognised as an almost common occurrence, and Schrenck, as far back as 1892, was able to collect over 600 cases which had been reported in the preceding *five years*. Since that date the literature on the subject has become immense; while the removal of a ruptured pregnant tube is now an every-day episode in surgery.

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[1] "Extra-uterine Pregnancy," John S. Parry, London, 1876.

The Cause of Tubal Pregnancy.—Tait in trying to explain how tubal gestation came about starts with the proposition that the uterus is the seat of normal conception, and that as soon as the ovum is affected by the spermatozoa it adheres to the mucous surface of the uterus. He maintains that the function of the ciliated lining of the Fallopian tubes is to prevent spermatozoa entering the tubes, and at the same time to facilitate the progress of the ovum into its proper nest. He maintains that any affection of the tubes that would cause destruction of the tubal cilia, such as desquamative salpingitis, would put the mucous lining of the tube into a condition resembling that of the uterus, and would be favourable to the development of the ovum in the tube.

Tait's idea that fertilisation occurs normally in the uterus had the support of Martin, Dührssen, and Bland Sutton; but later investigations have cast some doubt on this, and it is now suggested that the ovum and spermatozoon meet normally in the tube, and the ampullary portion is the site which is now in most favour.

Tait maintained that the cilia of the tube created a current which moved the ovum towards the uterus, while the cilia of the uterus produced a current which sent bodies upwards, and there antagonised one another in the upper part of the uterus. The observations of Hofmeir and Mandl have, however, shown that the current, both in the uterus and the tube, is towards the cervical canal. We know, however, that if an insoluble powder be placed high in the cervical canal the particles will be gradually moved up into the uterine cavity and along the Fallopian tubes to the abdominal ostium. We must, therefore, say that, in spite of the action of the cilia tending to make a down current, there is an up current which favours the progress of the spermatozoa towards the tubes and along them.

If we hold that normal fertilisation takes place in the uterus, then every ectopic pregnancy is due to fertilisation of the ovum away from the uterus; if, on the other hand, we hold that fertilisation generally takes place in the tubes, then every normal pregnancy is due to an ectopic fertilisation, and ectopic pregnancy is due to the accidental

retention of the fertilised ovum in some part of the tube or ovary. What, then, are the causes that will tend to a retention of the ovum in the tube? When we have answered that question we shall come close to the secret of ectopic gestation.

Schroeder, in 1872, Martin, in 1881, and Tait later on, thought that the most frequent etiological factor in ectopic gestation was an endo-salpingitis, which caused the destruction of the ciliated epithelium of the tubes and consequent cessation of the down current; though, as Bumm remarks, this view presupposes the presence of enough cilia to carry the ovum to its lodging place.

Innumerable observations since Tait's day have shown that many cases of tubal pregnancy have been preceded by pelvic inflammatory troubles, and a history of gonorrhœal salpingitis, or chronic salpingitis, may be found in over fifty per cent., if not more, of the cases. Tait, as we have mentioned, thought that the inflammation of the appendages led to the destruction of the cilia, and this allowed the spermatozoa to enter and fertilise the ovum in the tube. But against this theory are placed the observations of Whitridge Williams, Zedel, and others, who have shown that cilia often persist in spite of acute inflammation of the tubes, and that cilia can be demonstrated in nearly every pregnant tube, while Zedel actually saw the cilia in motion in several specimens which he examined in the fresh condition.

A salpingitis that would be so severe as to cause destruction of the tubal epithelium would be almost sure to close the abdominal ostium of the tube and prevent the entrance of the ovum, as Bland Sutton long ago pointed out.

Perhaps the correct explanation of the relation of salpingitis to ectopic pregnancy is furnished by Opitz, who found definite histological inflammatory lesions in two-thirds of his specimens, while he also discovered that the tips of many of the folds of the mucosa had become fused together, thus forming false diverticula, leading in some cases almost to the obliteration of the lumen of the tubes. These false diverticula run parallel to the direction of the tubal lumen and communicate with it, and so it is suggested that if a

fertilised ovum should find its way into one of these diverticula, it may develop there and so give rise to an ectopic pregnancy.

Since Tait's day much attention has been paid to the etiology of tubal pregnancy. Olshausen, Orthmann, Trochownick, Schauta Dührssen, Mandl and Schmit, Kustner, and many others have published observations in support of Tait's contention that inflammatory conditions of the tube were a cause of tubal pregnancy.

Freund and Abel have brought forth evidence to support the idea that persistence of the infantile condition of the Fallopian tubes was the cause of tubal pregnancy.

Leopold and Wyder have suggested that the presence of a polypus in the tube may be a cause, and the view was supported by Krusch.

Henrotin and Herzog described a case where the pregnancy had developed in an accessory tube, which did not communicate with the main tube.

Dührssen, in 1897, suggested that puerperal atrophy of the tubes was a cause; while Clarence Webster brought forth the theory that atavism—reversion to a lower developmental type of tube—might be a cause.

We need not mention the many other theories that have been put forth concerning the etiology of tubal pregnancy. Tait's original observations, concerning the relationship of salpingitis and ectopic pregnancy, have been abundantly confirmed, although, as we have seen, his method of accounting for an ectopic gestation in such tubes has not been accepted. The etiology of tubal pregnancy is not a simple matter and there is no universal cause for all cases, for in many instances neither the clinical history, nor the microscopical examination of the specimens, will reveal any tangible cause, and so ectopic pregnancy, as Whitridge Williams remarks, will continue to remain as great a problem to us as to our predecessors.

Classification of Varieties.—Tait next passes to the classification of the varieties of ectopic gestation, and says that he at once proposes to dismiss all previous classifications as inconsistent with the facts as they have occurred in his

own experience. He then enunciates a truth, now accepted on all sides: "It follows as a matter of course that all ectopic gestations must, in their origin, be tubal. A possible exception to this may be the impregnation of an ovum in its vesicle before it leaves the ovary."

To us this truth appears a very simple matter; but we must not forget Tait was the first to insist that all ectopic pregnancies were primarily tubal. Parry, in his work (1876), had tried to classify the 500 cases that he had collected: he thought that 214 were tubal, 27 ovarian, 29 primary abdominal, while 230 he *could not* classify. Once Tait had clearly pointed out reasons for his view that all cases were in the first instance *tubal*, then from that time on the subject became much clearer to every one.

Tait says that, in regard to the tubal cases, we must make a distinction between those cases where the fertilised ovum became attached to the wall of the tube, where it is free from uterine tissue, and those cases where the ovum cavity is formed by the distension of the tube at that part imbedded in the structure of the uterine wall; these cases have been called "interstitial," and he proposed to retain this term.

Tait then goes on to say:

> The process of development of an ovum in the tube, at any part of it, inevitably results in rupture of the tube . . . this tubal rupture takes two directions:
>
> (*a*) Into the peritoneum, which is the fatal form; and
>
> (*b*) Into the cavity of the broad ligament, a form which yields the variety of ectopic gestation which I propose to call extra-peritoneal.

These views expounded by Tait have been modified because we now know that the great majority of cases terminate at an early period by tubal abortion, after the capsular membrane—which is superficially analogous to the decidua reflexa—has been ruptured through.

The frequency of tubal abortion no doubt depends on the site of the implantation of the ovum. When the ovum develops in the ampulla of the tube, tubal abortion is the rule; if, however, the ovum is situated in the *isthmus*, intra-

peritoneal rupture is the usual result. There is no doubt that Tait thought that rupture of the tube into the folds of the broad ligament occurred much more frequently than it does. He also thought that it was only under such circumstances that extra-uterine gestation could go on to full term. It is, however, highly probable that when an intra-ligamentary gestation progresses, the tube does not rupture into the broad ligament, for if that occurred, in most of the cases the ovum would be destroyed. It is most probable that as the ovum develops it gradually stretches and erodes the wall of the tube in relation to the cellular tissue of the broad ligament, so that these tissues are opened up and expanded by the growing ovum, which lies in a sac formed in part by the tube, and in part by the broad ligament (Blacker).

Tait deals at considerable length with the subject of pelvic hæmatocele in its relation to tubal pregnancy, and by his masterly handling made the subject so clear that there has been no difficulty in understanding the subject since his day.

Tait was very emphatic that many extra-peritoneal hæmatoceles were to be explained by extra-peritoneal rupture of tubal gestations, and that when this took place such a case might become a fatal one in its later course by a second rupture of the broad ligament cyst, resulting in hæmorrhage into the peritoneal cavity; while in some cases the collection of blood might be infected and give rise to a pelvic abscess, and he remarks that he had already met with quite a large number of such cases.

When Tait comes to deal with intra-peritoneal hæmatocele he points out that it frequently arises from ruptured tubal pregnancy; but he makes the very important observation that fatal hæmorrhage can occur from the Fallopian tube into the peritoneal cavity, under circumstances when the occurrence of rupture of the tube by an ovum is out of the question.

Now this is a very important observation, because it has gradually dawned on those who operate on pelvic cases that many cases, where blood has been found in the pouch of

Douglas at the time of operating, are not instances of hæmatocele *due to ruptured pregnant tubes*. This fact has been brought out after a large number of tubes have been minutely examined under the microscope and no trace either of fœtus, or villi, or any decidual formation, could be discovered. The conclusion, therefore, that we must come to is that, while a ruptured pregnant tube is a fertile source of intra-peritoneal hæmatocele, the ovary and the tubes under conditions where pregnancy is proved to be absent can pour forth sufficient blood to cause a fatal hæmorrhage.[1]

Tait next deals with intra-peritoneal hæmatocele, and points out its connection with ruptured tubal pregnancy, and remarks :

> We come to the following conclusions : that in the great majority of cases of extra-peritoneal hæmatocele, even where due to ectopic pregnancy, the disease may generally be left alone, being rarely fatal, and that it is to be interfered with only when suppuration or extreme hæmorrhage has occurred. . That, on the contrary, intra-peritoneal hæmatocele is fatal with such almost uniform certainty that so soon as it is suspected the abdomen must be opened and the hæmorrhage arrested.

Experience has shown that Tait is not quite correct in his last conclusion. If we have a rupture of the isthmial part of the tube, even when the ovum and placental tissue is very small, we may have a fatal hæmorrhage in so short a time as ten hours ; on the other hand, when the ovum develops in the ampullary portion of the tube, we may have a very extensive intra-peritoneal hæmorrhage from which the patient may recover.

In the latter part of Tait's essay he deals with the fate of the gestation after rupture has taken place and the fœtus lives and develops.

At the time (1889) that he published his observations, he considered that a primary rupture into the peritoneal cavity seems to be almost necessarily fatal alike to mother and child, but where the rupture occurs into the cavity of the broad ligament, it may be followed by a continuance

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[1] Bovée, *Amer. Jour. Obst.*, March, 1918.

of the development of the child, and these only are the cases in which the child is permitted to reach a viable stage.

Recent observations show, however, that many cases, which before were classed as intra-ligamentary, were really instances of ruptured intra-peritoneal forms incapsulated in adhesions, and are now described as pseudo-intra-ligamentary forms : one of the guides by which these forms are recognised is that the ovary, which should lie in view in an intra-ligamentary form, is concealed beneath the adhesions in the pseudo- form.

Tait at this period had not met with a case where the rupture had occurred into the peritoneal cavity, the placenta remaining in the tube and development continuing—this is termed tubo-peritoneal gestation. When, however, we were assisting Mr. Tait in Birmingham in 1891, he encountered his first example of this condition. Not long after this Webster published a monograph on the subject,[1] and Taylor,[2] in his work on "Extra-Uterine Pregnancy," described a case he had operated on, and found, as Tait had found in his case, that the amnion was intact. He says :

> On careful examination of the abdominal cavity in this case conducted by myself and my colleagues, Mr. Martin and Mr. Jordan, it was found that a transparent membrane (the amniotic sac) surrounded the fœtus and protected it in every direction, and that this was invisible on the surface of the intestine, but could be demonstrated as a film passing from coil to coil and completing the sac in which the pregnancy had developed.

At the time when Tait wrote his Essay, no case of ovarian gestation had been proved to exist. Tait did not deny that ovarian and tubo-ovarian pregnancy were possible, he simply said at this period that he had not encountered instances of the condition, and that ovarian pregnancy would have no kind of clinical interest or importance not already decided upon in the case of pregnancies in the free part of the tube.

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[1] Webster, "Tubo-peritoneal Ectopic Gestation," Edin., 1892.
[2] "Extra-Uterine Pregnancy," 1899.

We now accept ovarian gestation as a demonstrated pathological entity.

Tait would not admit that primary abdominal pregnancy was possible, because he used to say that, from what he knew of the digesting powers of the abdomen, no gelatinous fœtus of the tenth week could exist there, let alone an ovum that had escaped, after being fertilised, into the peritoneal cavity. However, Hirst and Krupe, in 1908, and Gröne, in 1909, have described specimens which lend some support to the idea that an early complete ovum might be able to escape into the cavity of the abdomen and develop on the peritoneum, in spite of the great improbability of this view.

Treatment.—Tait's rule to operate as soon as the rupture in the tube is diagnosed is almost universally followed. Robb, in 1907, advocated deferring operation while the patient was profoundly shocked, and waiting until the general condition had improved. Few agree with him, but we have adopted this attitude during the last twenty-five years in most of the cases we have seen, and have lost but a single case out of 100 operations for ectopic pregnancy in our hospital practice—and that was a septic case which had ruptured a month previous to the operation. We do not believe that it is a wise thing to place a patient who is pulseless on the table to operate on her; many of the cases operated on have lost comparatively little blood, and if they are kept warm and receive intra-venous injections of saline, or gum water, they are better subjects for an operation in a few days' time than they are when pulseless. We are aware that this view is not in favour at present, but we base our practice on personal experience, and on the following grounds: That rupture of a tubal gestation into the folds of the broad ligament never calls for immediate operation; that tubal abortion is seldom fatal; and that the only cases of tubal rupture that call for consideration for immediate operation are cases of intra-peritoneal rupture of the isthmial portion of the tube—one case in twelve—when the hæmorrhage may be so severe that a fatal result may even occur before any operation can be performed. In such cases the

patient must be given the benefit of an operation, for if the operation is not performed death is, as a rule, but not always, inevitable.[1] Thus we come to the conclusion that in 88 per cent. of ruptured tubal gestations the operation may be delayed for some days with benefit to the patient; immediate operation is no more *imperative* in ruptured tubal pregnancy than it is in ninety per cent. of cases of appendicitis! Experience must be our guide whether we operate at once, or maintain a "watchful armed neutrality in the desperate cases," as Howard Kelly remarks.

When now we come to assess the amount of Tait's contribution to our knowledge of ectopic gestation we must award him much honour. Why? Because he was the first surgeon who ever ventured to open the abdominal cavity with the object of placing a ligature on the ruptured vessels, and so save the patient from death. If Tait had never done anything else than this one operation it would have given him a place among the *Immortals* of surgery. Others had glibly pointed out what ought to be done, and they were right; but it remained for Tait to do what no one else had attempted to do.

This is the first, and perhaps the most obvious, reason why Tait's name will be for ever connected with ectopic pregnancy; but in our opinion there is a second, and equally important, reason why his name should always be remembered in this connection. Tait was the first to point out that all ectopic gestations are primarily *tubal;* the few ovarian gestations now acknowledged did not affect Tait's great generalisation; and we must remember that he always said that there was no *à priori* reason why ovarian pregnancy should not occur.

The recognition of the fact that all ectopic gestations were primarily tubal was the foundation of the correct pathology of the subject. Once it was realised that the Fallopian

[1] Whilst writing this chapter we operated on a case of rupture taking place in the isthmial portion of the tube; the patient had been left four days and had lost over a pint of blood, but she recovered. Twelve months later she returned with similar symptoms, and we opened her at once, only to find that the remaining tube had become distended with pus; on being examined it was shown that it was a tubercular tube.

tube was the starting point of all the pathological mischief, the subject at once was placed on a scientific basis. Thirty years of observations have confirmed the truth of Tait's acute generalisations, and while we now admit that ovarian gestation is a fact, we cannot yet admit that primary abdominal gestation has been satisfactorily proved.

Some of the points that Tait was emphatic about have been objected to, and others have been upset. This, of course, is not to be wondered at. He was a pioneer in the field, and he formed conclusions in his virile way from a limited experience, consequently some of his conclusions were imperfect, and some not true, so that they have had to be modified or rejected.

He was very emphatic that the uterus was the normal place for the spermatozoa to meet the ovum on its downward journey. This at least had the virtue of simplicity. Evolution had given to the human female a place where the young should be developed: it seemed a strange proposition to put forth that, in spite of this last highest effort of development, we should discover a variety of development which brought death to the subject, and that this mode of development should give rise to the theory that the uterus was not the proper place for fertilisation, but that this had to be done in a by-way in every case before the uterus could tolerate the presence of the prepared ovum. In spite of all that has been urged in favour of the tube as the usual site for fertilisation of the ovum, there are many who hold on *à priori* grounds with Tait that the uterus is the usual site for the fertilisation of the ovum. On the other hand, the outer extremity of the tube is held by not a few to be the usual site; but the matter is still *sub judice.*

Still another point that Tait was most emphatic about. He maintained that if a tube contained a gestation it must rupture. If it did rupture, it either did so into the peritoneal cavity or between the folds of the broad ligament. Tait, when he published his lectures on the subject, had not realised that tubal abortion frequently occurred, and we can well remember that in 1892, when the subject was

discussed, he maintained that tubal abortion was in reality a species of rupture. This fact, after thirty years, is now generally admitted, even if we have to qualify the expression "rupture" by saying that tubal abortion must necessarily be preceded by internal rupture of the gestation sac before the ovum can be free in the tubal lumen, or find its way through the tubal ostium. While admitting that tubal abortion is a rupture, we must, at the same time, say that the surgical importance of this form of rupture is much less than the surgical importance of rupture of the isthmial portion of the tube into the peritoneal cavity. Tait thought that the tube either ruptured into the peritoneal cavity, the resulting hæmorrhage of such a rupture being fatal, or else the rupture took place between the folds of the broad ligament. He was quite right in holding that these ruptures did take place, but we now know that rupture into the broad ligament folds is not common, and that isthmial rupture occurs in only one-eighth of the cases, while tubal abortion, and tubal mole formation, are the most usual ending of a tubal gestation, and that these are seldom fatal.

With regard to the fate of the gestation, when rupture does take place, Tait was quite right in his observations, if we put in the proviso that, when the gestation leaves the tube and continues its existence in the folds of the broad ligament, no rupture with hæmorrhage takes place; the walls of the tube expand and give way, but this is *not accompanied by hæmorrhage.* As regards primary abdominal pregnancy, it has not been proved to exist; while all abdominal gestations are either secondary, having come about by rupture of the tube and expulsion of the fœtus surrounded by its amniotic membranes, or they may be tertiary, as Tait pointed out, being due to a rupture into the broad ligament folds and then a rupture into the peritoneal cavity.

XXIX

Diseases of Women and Abdominal Surgery

1889

In 1889 Tait published the first volume [1] of a proposed two-volume work on the diseases of women and abdominal surgery.

He opens the work with the following Preface :—

> In the preface to my first work of this kind, which appeared in 1877, I urged a "plea that any new effort to extend our acquaintance with the special diseases of women deserves at least to be received with patience." This plea was, to a very large extent, disregarded, and the method in which my little effort was received was speedily manifested in two wholly different and, I am bound to state, wholly unexpected directions. The first direction to be indicated is, of course, the most extended, and that consisted of ridicule, abuse, and misrepresentation.
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> The second direction was to me, however, far more important, and I soon ceased to trouble about the other. A small band of younger and more earnest men came to see and judge for themselves, and were speedily converted to the belief that a new field of work had appeared; and many, who had started with a sneer at the "Birmingham School of Gynæcology," remained to speak of it with respect, to adopt its methods, and become its earnest and progressive disciples.
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> It is curious, as a mere little bit of history, to read the reviews of my work of 1877, and to read, as a contrast, the impression it has created in ten years, as indicated in the pages of such a masterly work as Greig Smith's book on Abdominal Surgery, 1887.
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> What I am now saying must, of course, appear as the outcome of egotism, but I must give it as the reason and explanation of much I have to include in the present work, and as a justification for my expressions of unbounded gratitude to the members of the American medical profession

[1] "Diseases of Women and Abdominal Surgery," vol. i., Leicester: Richardson & Co.; Philadelphia: Lea Brothers, 1889.

who have done so much to aid me in what has been accomplished. To many continental friends—Italian, French, Danish, and a few German—I have also a debt of acknowledgment to offer for their support, and their candid acknowledgment of the value of what they have seen for themselves.

During the period of these ten years gynæcology and abdominal surgery have made marvellous advances. The old-fashioned mechanical school—the teaching of the speculum, the sound, the caustic stick, and the pessary—has been practically killed, and an advanced eclecticism now prevails. This has largely grown out of the wonderful revelations obtained by the experiences of operative surgery. In 1877 Spencer Wells left ovariotomy where it had been for half a century before him, with a mortality of one in four. Now it has a death-rate of a little more than three in a hundred, and this fact alone has given an impetus to, and a facility for, the alleviation of suffering in other directions, of the value of which the most enthusiastic estimates that have yet appeared probably fall quite short.

The present work professes to deal with "Diseases of Women and Abdominal Surgery," a title which records the historical fact that the evolution of abdominal surgery has proceeded entirely from the necessities of the special diseases of women, and their combination is now so complete that it is unlikely they will ever be again separated. The day has gone by when the treatment of pelvic and abdominal diseases, so prevalent amongst women and relatively so rare amongst men, was regarded as a mere appendix to the work of the accoucheur. It has gone as completely as the day when diseases of the eye were dealt with in a chapter appended to a text-book on the practice of medicine. Gynæcology and obstetrics are now happily severed, and this division of labour has resulted in enormous advances for both.

In the plan of the present book I have followed the scheme of my own experiences in as condensed a form as possible. If the present edition is to have any value, it can arise only from what I have to say of my own work. If I fail there I do not care to succeed by padding it with extracts from the work of others. Nor can I imagine it possible that any modern gynæcologist, whether he accepts my views or not, could conscientiously say that my work has not been extensive enough to entitle me to express my convictions concerning it.[1]

In place of a table of contents Tait gave a "table of the organs concerned," commencing with—(i) Mons veneris; (ii) Vulva, with various sub-headings; (iii) Vagina,

[1] Birmingham, April 16, 1889.

Urethra, and Bladder; (iv) Uterus; (v) Broad Ligaments and Mesentery. The second volume, which he never completed, was to have dealt with the Fallopian tubes and Ovaries, Pelvic Bones, Liver and Gall-bladder, Kidneys, Spleen and Pancreas, and the Intestines, and lastly, the Breast.

The work was largely a collated collection of his previous papers; it also embodied his former work, "The Diseases of Women."

We do not propose to go minutely through the various chapters in this work, but merely to pick out some matters with which Tait's name is intimately connected, and give his views on those subjects. The book was well received; naturally objections were taken to some of the author's views, and with justice; for instance, Tait held the Bimanual method of examination in some contempt: time has elevated this method of examination to the very highest rank, it holds an essential part in gynæcologic diagnosis.

In the fourth chapter, which dealt with the uterus, Tait gave a full explanation of his views on the pathology of uterine myoma. He was always accustomed to throw fibroid tumours of the uterus into two classes. In one class were placed the soft œdematous myomata, and in the other kind the multinodular myomata.

A soft œdematous myoma, he said, was always solitary, and it could be enucleated easily. When a uterus with one of these tumours is removed and cut into, a large quantity of serum drains away in a few hours so that the tumour will diminish very considerably in weight.

Very different are the naked-eye appearances of a multinodular myoma. It may be submucous, subserous, or interstitial, and a uterus containing multinodular myomata is irregular in outline, and has one or more knobs of the growth projecting from its surface.

> The great bulk of cases of multinodular myoma are seen after the age of thirty-five, perhaps we might almost say forty; and though, as I shall afterwards show, their presence in the uterus may delay the menopause indefinitely, yet when it does occur, either naturally or as the result of operative interference, they are arrested in growth, and dwindle.

In fact, when the appendages are removed for multinodular myoma under the age of forty, the tumours generally disappear entirely.

But the facts observed in connection with soft œdematous myoma are very different. It is no regarder of age. The largest I ever removed, sixty-eight pounds, grew entirely after the menopause.

Tait drew attention to the fact that we may meet with this kind of tumour in young women of eighteen years of age.

With regard to the removal of the uterine appendages for the soft œdematous variety, he wrote: "I am almost being forced to the conclusion that removal of the uterine appendages in this disease, though it may be perfectly successful in arresting menstruation, may yet entirely fail to arrest this growth of the tumour."

Tait draws attention to the fact that hæmorrhage is almost exceptional in soft œdematous myoma, and gives us the history of how he first came to remove the uterine appendages for the treating of uterine myoma. He says:

The idea of the removal of the uterine appendages for the treatment of uterine myoma occurred to me after the death of two enucleation cases in one week in the year 1871. I was horrified at the catastrophe, and as I witnessed the post-mortem of one of the cases I was struck by the exaggerated size of the tubes and ovaries. This association of diseased conditions of the appendages with myoma has been a frequent experience ever since, and I think there is some relation, as cause and effect, between the two conditions. It struck me as being so at the time I speak of, and remembering that in a case where I had performed double ovariotomy some months before, in a woman blanched by uterine hæmorrhage, I had secured the gratifying result of completely arresting that symptom, it struck me that I might apply that experience to bleeding uterine tumours. I had no opportunity of putting my idea into practice until July 27, 1872. That woman was cured, but not immediately, as I fancy I did not do the operation as I do now; but she had complete arrest of the hæmorrhage in a few months, and is still alive and well. Since then, and up to the end of 1888, I have performed the operation 272 times, with 12 deaths,[1] giving an average

[1] "General Summary of Conclusions from a Series of One Thousand Consecutive Cases of Abdominal Section, 1884;" *ditto*, "Second Series of One Thousand," *British Medical Journal*, 1888.

mortality of 4·41 per cent. In my first series, the mortality was 7 per cent. as against 2·03 per cent. in my second series, and if I could secure that all my cases reached me before they were half-dead from mere anæmia, I think the mortality would not be more than 1 per cent. The whole of my experience in every department of abdominal surgery is one continuous outcry against delay.

On May 24, 1881, I read a paper on my first thirty cases of the operation before the Royal Medical and Chirurgical Society; but my statements only provoked incredulity, and that conservative body did not even think it worth while to publish my paper. I received their formal thanks without emotion, and sent my paper to America, where it was published in the *American Journal of Medical Sciences*, January, 1882, and in the New World it at once received the attention which I knew the facts merited. I can now speak calmly of the action of the London Society, but I felt very bitterly about it at the time.

In a paper read at the Cardiff meeting of the British Medical Association in 1885 (*British Medical Journal*, August, 1885), I published in detail the secondary results of my first fifty cases of this operation in which recovery had taken place. In all of them a period of more than two years and a half had passed since the operation, so that there was an interval between the treatment of each case and the report of it quite long enough to give a satisfactory statement of the secondary result. The first point, in that paper, was to show that removal of the uterine appendages for myoma, when properly performed, is not a fatal operation, but one, indeed, with hardly any mortality at all, even when the tumours are large and the patients are brought almost to death's door by excessive hæmorrhage. I therefore gave a list of fifty-eight cases in which I had operated between January 1, 1884, and July 21, 1885, without a single death; and I selected that period not because I had experienced a heavy mortality before that, but because it was the latest date up to which my practice had been published in detail. In the series published up to the end of 1883, there were fifty cases of removal of the uterine appendages for myoma with two deaths, so that up to August, 1885, there was a series of 108 cases, with two deaths. Since then, from July 21, 1885, up to the present date, I have had 154 cases with only two deaths, so that the mortality of the operation in my hands up to the present moment of publication, exclusive of the earliest work, which in no way represents the *mortality of the operation*, gives only 1·53 per cent. Adverse critics have been delighted to rake up my early cases, in which, with less than a score of cases, the mortality was

> nearly 25 per cent. ; but I need not say that, as I originated this proceeding, I have had to bear the burden of the blunders inseparable from ignorance—blunders which have helped me not only to mend my own ways but also to mend the ways of those who came after me, and who have forgotten to credit me with the better results which my misfortunes provided for them.

Tait concluded his consideration of the subject by giving a list of 262 cases in which he had removed the appendages for myoma with only four deaths.

Tait very properly pointed out that " the terms fibroid, fibrous, and fibroma, as applied to uterine tumours, are, I believe, so completely erroneous that they should be banished from pathological nomenclature."

Abundant microscopic examinations had shown him that he had not met " one in which simple fibrous tissue played any but a very subservient part."

His aim was to make pathologists believe that there were two kinds of uterine myoma—the " soft œdematous myoma," and the " multinodular myoma."

He drew attention to their different microscopic structure ; to the fact that the multinodular variety were met with most usually after the age of thirty-five ; that their presence in the uterus may delay the menopause, yet when it did occur, there was an arrest of growth and often a dwindling of these tumours. On the other hand, the soft œdematous myoma occurred in quite young women, or it might be found growing in women after the age of fifty or sixty years, and while the multinodular myomata often were accompanied by profuse hæmorrhage, with the soft œdematous there was often no hæmorrhage to speak of.

This classification of myomata is discarded by modern pathologists, because they regard all fibroids as of the same " breed," to begin with. What Tait describes as a *soft œdematous myoma* they state is nothing more than a condition of degeneration of the original fibroid.

According to this view, then, a myoma may be classed as corporeal, intra-ligamentary, or cervical ; while the corporeal ones may be further subdivided into the subserous, the

interstitial, and the submucous, according to the position they occupy in the wall of the uterus.

As to the corporeal [1] ones, it is now held that all arise as intra-mural growths, and if they start close to the serous covering they grow and become subserous; if close to the mucous coat, they become submucous. Their future fate then depends on processes of degeneration, and the result of these processes gives us fibroids which Tait believed to be of essentially different natures. The clinical facts that he had accumulated gave him a large measure of justification for his views, but the modern microscopists will not give him any quarter; and accordingly we have to accept the view that Tait's multinodular fibroid, and Tait's soft œdematous fibroid are of the same breed, and the difference in the microscopic appearance of a section of any myoma depends almost entirely on the degree of hyaline degeneration that is present. Accordingly, in hard myomas, among which would be Tait's multinodular myomata, we may have hyaline degeneration that can only be discovered by the microscope. In others the degeneration is more pronounced and is visible to the naked eye. In a third class—here we place his soft œdematous fibroids—there is advanced degeneration showing liquefaction and the formation of minute cysts; while in a fourth class there is a very high degree of degeneration with the formation of large cysts, and among these we encounter the fibro-cystic myomata.

The observation that Tait made, that since myomata appear during sexual life it is natural to infer that "ovarian activity" has some intimate connection with the growths, appears to be quite correct. If the ovaries are removed from a patient, say of the age of thirty-five, having one of Tait's multinodular myomas, the patient often ceases to lose any blood, and the myoma may shrink to a small size. On the other hand, this does not happen if the patient has one of Tait's soft œdematous myomatas, as he pointed out himself. He thought the reason was that the tumours were

[1] Tait makes the following mistake: "I never saw a nodule having its origin in the true cervix, and I regard myoma of both kinds [multinodular and soft œdematous] as essentially a disease of the body of the uterus."

of different " breeds." We now give the explanation that the soft œdematous myoma of Tait is one that was undergoing hyaline degeneration, and when once that process is well established " ovarian activity " has no effect on the myoma ; and it is for this reason that we may find that the myoma goes on growing after the menopause.

Finally, we now may say that what Virchow described as Myoma mucosum, and was later on described by Tait as the soft œdematous myoma, and which was considered by Virchow as a myxomotous degeneration, is now regarded as being a liquefactive stage of hyaline degeneration.

In the seventh section Tait enters fully into a consideration of the Fallopian tube in relation to menstruation.

With regard to menstruation Tait held firmly to the idea that the Fallopian tubes were the starting point of the process. He says :

> All that I have said further in the shape of a tubular theory of menstruation is that the tubes are the starting point of the process. I never said that they were the cause. That they are the starting point is proved by the clinical fact that the characteristic symptom of tubal occlusion and distension is premenstrual pain.
>
> If the tubes, therefore, are the starting-point of the menstrual phenomena it is not unreasonable to suppose that their removal will be a very effectual method of stopping it altogether, just as you stop the growth of a fir tree by cutting off its leader—and it is a clinical fact beyond dispute. But I never said, I never believed, that the tubes caused menstruation any more than the governors of a steam-engine can be supposed to drive the machinery.
>
> I do not see how it is possible to advance any speculation as to the cause of menstruation other than it can exist in a nervous mechanism, and there, for the present, we must leave it.
>
> The purpose of the phenomenon is more intelligible, however, and unfortunately the cause and the purpose have been inextricably confused by most writers, especially Dr. John Williams. Confining ourselves to the purpose, we are at once attracted to Dr. Aveling's nidation theory, and when this is read in the light of the brilliant researches of Mr. Bland Sutton and Dr. Arthur W. Johnstone, I think the answer is clear and complete.
>
> It is a remarkable fact, and one of great interest in the

history of the development of this most interesting question, that the papers containing the result of the work of these two observers were both communicated to the British Gynæcological Society on one evening (June 23, 1886), and that without any knowledge on the part of either that the other worker had been making such investigations, or had a paper ready on the subject. Yet to read the two papers one after the other, their interdigitations are so extraordinary that it would seem as if they had been in collusion. Their corroborations, therefore, made their evidence quite overwhelming.

Their main conclusions, first, for the acceptance of Dr. Aveling's theory of nidation, and their qualification of his description of the process of denudation, form really the basis of the advances they have made. Thus, Mr. Sutton qualifies the usual definition of menstruation as "a periodic discharge of blood from the uterus" by the very important addition, "accompanied by the shedding of the epithelium of the body and the fundus, as well as that lining the utricular glands near their orifices." He says the epithelium of the cervix does not participate in the changes, and that the Fallopian tubes remain passive so far as this mucous membrane is concerned—a most important fact this latter, as will by-and-by be seen when I discuss the pathology of extra-uterine pregnancy.

Dr. Johnstone completely confirms the facts of Sutton by saying of his examinations of the mucous surface of menstruating uteri, that in them he could find no lesion whatever, "except the stripping off of the epithelium lining their cavities and the outer ends of the follicles"—confirmation almost word for word.

The views advanced by most writers on this subject, previous to Sutton and Johnstone, were that the mucous lining the uterus was thrown off almost in its entirety, and curiously enough, they both select Dr. John Williams as the chief object of attack and blame for this error, and with one consent they show that the error was due to ignorance of the proper methods of, and subjects for, examination. Sutton says that, "if due care be taken in the process of hardening the specimens, the destruction of nothing but the lining epithelium can be seen; but that if the pieces of the uterus be badly preserved, or a long time elapse before the parts are placed in the hardening media, the changes described as denudation may be easily made out," that, in fact, the appearances described by Dr. John Williams were of his own creation.

We are, therefore, driven to Sutton's conclusion that to call any hæmorrhagic discharge from the uterus a "menstru-

ation " is a blunder, and it certainly is one to which we owe much of mistaken pathology and equally mistaken treatment of diseases of the uterus. He says that " under the term menstruation physicians generally include almost any escape from the uterine passage," whilst from the words I am quoting it is quite clear that the main factor of the process is the preparation of this uterine mucous surface for the retention of the ovum, after its fertilization, by the denudation of its epithelium. The loss of blood accompanying this (as is clearly shown by the researches of Johnstone on the wombs of animals) is an introduced accident. Pregnancy may occur without it in women, but without it the retention of a fertilised ovum seems impossible, and this conclusion sheds a flood of light on the facts of intra-uterine disease which were, without it, wholly unintelligible. A normal adult uterus, therefore, menstruates—that is to say, it denudates—once a month, and then, if a mature ovum is met in its cavity by healthy spermatozoa, the ovum is fertilised and retained; a simplified view of the whole question which commends itself at once as likely to be on the road to truth, if it be not the whole truth.

On the other hand, diseased uteri have hæmorrhages without denudations; infantile and senile uteri have neither hæmorrhages nor denudations, and therefore pregnancy in them is impossible—conclusions which are parallel with the facts alike of health and disease. Finally, the implantation of a fertilised ovum can take place only on a healthy mucous surface, freshly denuded; and here we have the explanation of the facts of the dating of pregnancies, and the story so far is complete.

But the interesting work of these two authors has much more to tell us. " Before menstruation," says Sutton, " the (mucous) tissue is everywhere infiltrated with rounded and irregularly-shaped cells, which, for convenience, may be denominated the corpuscular element. These cells diminish very considerably in numbers after the menstrual flow has ceased, to reappear immediately before the next period. It is exceedingly probable that the corpuscular element is due to the increased quantity of blood in the organ." Johnstone confirms this, and gives a beautiful drawing of the corpuscles, which I here reproduce, and another from a menstruating uterus, where the whole process is seen. " In it you see everything—the epithelial covering freely developed; the fine threads becoming granular, and the corpuscles becoming more numerous."

Both Sutton and Johnstone agree in the statement that the only change in the mucous layer of the lower animals is one of simple turgescence, with increase of corpuscular elements.

In none of the higher apes was Sutton able to detect anything approaching to denudation. So that he was led to say that there was nothing like true menstruation occurring in these animals. I think, therefore, it would be better to reverse the conditions of Mr. Sutton's definition of menstruation, and say that it consists of " a shedding of the epithelium of the body and fundus of the uterus, as well as that lining the utricular glands near their orifices, accompanied usually by a periodic discharge of blood."

Johnstone has carried his observations somewhat further than the point at which Sutton has arrived, and up to this point they entirely agree. The further consideration of this most interesting question on the lines upon which I propose to take it, is chiefly based upon Johnstone's observations, which I have no hesitation in characterising as the most brilliant contribution to this subject which this century has seen.

Johnstone starts on a generalisation which is really the conclusion of his observations, at once startling and revolutionary. But upon the mature consideration of his communication, and still more upon what I can speak from personal investigation of his sections and their method of preparation, I can have no doubt at all that it is the correct one. Briefly stated, it is that, " in the ordinary acceptation of the term, the endometrium above the internal os is not mucous membrane, but belongs to the so-called adenoid tissue, and that menstruation is for it exactly what the lymph stream is to the lymph glands, or the blood current to the spleen." By menstruation, of course, here he means the hæmorrhagic process, though it is quite likely that the denudation—that is, the stripping off of the epithelium—is equally the result of the effusion of blood. It is then carried away, along with the corpuscular elements, into and by the stream. The development of this corpuscular element, according to his observations, is exactly what he has established to be the fact in the blood-glands, such as the spleen, thymus gland, etc. The corpuscles are developed, not by subdivision of cells, but by generation de novo from the granule in sustentacula threads, and the drawing which I have reproduced gives an admirable illustration of the facts—facts which I displayed in a paper which I read before the Royal Society of London in 1876, on the " Minute Anatomy of the Umbilical Cord." That paper displayed exactly the facts of his drawing, though I am bound to say I did not understand them, nor was there any attempted interpretation on my part in the least degree allied to that of Dr. Johnstone. The only consolation that I have is that, by the early use of a high-power immersion lens, I did see these things, and I now know

how it is that the umbilical cord grows without a direct vascular supply.

The story of the development of the endometrium has been completely worked out by Johnstone, and the story is a most satisfactory one.

Speaking of his second illustration he says, "A casual glance would make us think that her endometrium (a child of eleven years of age) was nothing but a thick dense layer of fibrous connective tissue between the muscular wall and the very slight, badly-developed row of round cells which take the proper place of the lining epithelium. But, examined with high power, it can be easily seen that this is not ordinary white connective tissue, for its elements have no proper, large, well-formed nuclei, which fibrous tissue of this age always has, but the fibres are composed of a fine thread-like tissue, which adenoid organs alone possess. A few corpuscles are dotted through its meshes, and the little pits which form the glands of the future are conspicuous for their shallowness." The conditions here described were entirely confirmed by the investigation of a section of a case of infantile uterus which I was enabled to place at Dr. Johnstone's disposal for his research. We see, therefore, that this remarkable condition of arrest of development, and all the inherent sufferings attached to it, are precisely those of the period from which it gets its name. The nerve mechanism, as well as that of the vascular supply, obliges the uterus to perform work for which it is not fitted, and the result is intolerable suffering.

The next illustration is the endometrium of a girl of thirteen who had menstruated twice, and there may be seen the epithelial lining freely developed. The fine threads have become granular, the corpuscles are more numerous, and the whole is much richer in the so-called protoplasmic elements, while the dense bands still remain, and any one familiar with the adult tissue can see that this is a near approach towards it.

The fourth illustration is from a full-grown, developed woman of twenty, in whose endometrium there are all the conditions which are familiar—bands, plates, threads, and everything studded with their protoplasmic outgrowth, ready to take up and nourish the ovum by forming its placenta. But at this stage it may be said that the endometrium has reached the highest point of its unimpregnated development. This remains through the period of life during which puberty is passed. The ruin to which old age brings it is well shown in the last illustration, where the absence of the large bundles and the thinness of the whole membrane show its senile decay. The fibrillar structure seems wasted, corpuscles are

few, and the utricular follicles shrunken and scanty. Precisely as we might expect, the condition of infantile uterus gives the microscopic appearances of arrest of development in its glandular constituents most thoroughly, and the final proof of Johnstone's views is derived from the examination of such an example. "In a case of arrest of development, of which I now have in my possession the specimen, the uterus measures barely two inches, and the woman from whom it was removed, though aged thirty-nine, and an unusually large and fine-looking woman, had always great irregularity and difficulty in menstruation. It was one of those queer and formerly inexplicable cases when sometimes there would be for a period of several years total arrest of menstruation; at other times she would be comparatively regular for a year or so. The microscope showed her endometrium to be between the conditions shown in my paper on the menstrual organs of eleven and thirteen. So that, after all, the poor creature had gone through life trying to menstruate with an endometrium that was not far removed from the condition of the pig's, but with the one great exception that she had little or no lymphatic stream with which to relieve the tension. This adenoid view of the endometrium and its method of forming the placenta at once explains to me the mechanism of membranous dysmenorrhœa and the formation of uterine moles. By some irritation it gets a false start, and the attempt is made at a rudimentary placenta, so that, after all, they are merely slight perversions of a physiological function. The cure of chronic and corporeal metritis by the curette, chemical irritants, actual cautery, and other powerful revulsives is accomplished not only by the mechanical removal of the indurated tissue, but by these proceedings the tissue is returned to the medullary state, and from the fresh start the tissue resumes their normal condition.

Concerning the functional relations of the uterus and ovaries Johnstone thus most aptly expresses himself, and confirms, even in the mere statement of the analogy, the facts which Ritchie, Reeves Jackson, and a host of others have in vain been trying to persuade our professional brethren are really true. He says ("Transactions British Gynæcological Society, 1887," p. 390): "The uterus, instead of being a mere appendage to the ovary, is as much a specific organ as the ovary itself, and its independence and totally distinct functions we now as thoroughly understand as we do those of the matrix of the egg. Its association with ovarian activity is that of two separate departments of an army, each of whose objects can be attained. They are both controlled by branches from the sympathetic system, and

instead of their actions being determined by each other, either orders come from that higher power which controls all functional activity."

We have, then, the story of the endometrium, that it is formed, as Johnstone says, remarkably like the hair-follicles of the face in the male, whose first products herald the approach of adult life; probably they have been there before that, lying in quiescence. They are active during the period of full sexual life, and then, in the period of senility, they undergo decay and disappear.

The endometrium is perforated at that time by the so-called glands, whose ramifications convert the whole into a sponge-like mass, all of whose channels lead into the cavity of the body. Its epithelial covering consists of a single layer, which slips into every reduplication of the glandular canals, and thus gives a soft protecting coat to the soft protoplasmic tissue. We know by clinical experience that change is effected in this by disease, and the results arising from it concur absolutely with his microscopical description, and the conclusions to be determined therefrom. When by chronic villous endometritis this endometrium becomes too pronounced, we have a hæmorrhagic discharge and profuse menstruation which is easily cured by the curette. We have also, as I shall have, further on, occasion to speak of, a complete clue to pathological changes from this view of Johnstone's. We have a clue, I believe, to the cause of the frequency of cancer of this organ, for the view I have held and taught of cancer of the uterus for many years past is that it is essentially a disease of the endometrium, beginning there uniformly. Whatever its changes may be in its progress, its inception is always in this layer. Johnstone has continued his researches into the comparative anatomy of the endometrium, and his conclusions are equally interesting. He finds that in the ruminants the epithelial covering is like that of the human uterus, except that it is much thicker, often containing as many as eight or ten layers, with this remarkable point of difference—that there is an immense number of lymph canals, whilst in women these are very few, mere evolutionary survivals. In the cow and sheep he has found that not only the space between the cytogenic layer and the muscular wall is taken up with a rich layer of lymph canals and intercellular spaces, but that the plexus of vessels themselves is richly supplied with lymph radicals and perivascular sheaths, such as are found abundantly in the brain and optic nerve of every animal. In fact, the arrangement gives exactly the impression of a modified lymphatic gland. In the sow, on the contrary, the lymphatics are not nearly so rich, and the relations between the muscle and the

endometrium are a nearer approach to what is found in women. With the exception of the very thick epithelial coat, the internal aspect of a sow's uterus is merely a reproduction of what is found in the human being. But the parenchyma is that of the child's uterus before menstruation, not that of the adult.

The difference between the conditions found in women and those of the lower animals Johnstone attributes to the erect position of man. Thus he says, "In two of the ruminants that I have examined it appears that nature has supplied the endometrium with an abundant lymph stream which in the unimpregnated state washes away the ripe material to the general circulation exactly as it does any other lymph corpuscle. But in women, where, on account of its erect position, the uterus has to depend on the tenacity of its own fibres for the preservation of its shape, no such thing as loose tissue of a lymphatic network can be depended upon. So, to preserve the integrity of the uterine wall, the emulgent stream is poured into the cavity of the body, and got rid of through the vagina."

Here, then, we have the explanation—the only thing which was wanted for the capping-stone of the edifice—of the introduction of this process of menstruation in the human being, and of its supposed appearance in the higher apes. I do not think it is going too far to apply this explanation to the excessive amount of suffering which seems to have been introduced into the whole scheme on the appearance of the erect position, and the extremely peculiar functions of the human uterus, which oblige it to be one of the main factors of the sufferings induced by the process of civilisation.

"Finally," says Johnstone, "the microscope confirms the long known clinical experience that the nest is always ready, but that, like everything else about a woman, there are times when it is the better for a clearing out. The well-known fact that pregnancy is most apt to occur directly after menstruation has always been a stumbling-block to the acceptance of the denudation doctrine, for I am sure that no other female will lay in a nest directly after she has destroyed it. Therefore the simplest definition of menstruation is a periodic wasting away of those corpuscles that are too old to make a placenta. Stripped of its epithelium, the endometrium reminds one very forcibly of the sponge-like tissue of the lymph glands, and it is very easy to see how like the lymph stream the menstrual blood creeps through its meshes and washes away the corpuscles exactly as it is constantly doing in the spleen. It may be urged against the views I have advanced that if this is an independent organ, how is it that the removal of the uterine appendages puts a stop

to its functions? My answer is, How is it that a section of the chorda tympani practically destroys the sub-maxillary, or the excision of a piece of the great sciatic eventually results in the shrinkage, if not in the sloughing, of the whole limb? "

In a more recent paper, read at the meeting of the American Gynæcological Society in 1887, Dr. Johnstone continues his remarks on this most interesting subject.

One can only say in conclusion, concerning this remarkable doctrine of Johnstone's, that its very simplicity will form the greatest stumbling-block to its acceptance, for the trouble has been all along with physiologists and pathologists in the past that nothing which is not circuitous, complicated, and in the main incomprehensible, will suit them as an explanation of the processes by which nature has arrived at her results. The strongest objection which was ever advanced against Darwin's theory of the origin of species was the sublime simplicity of the process by which it was accomplished. So I expect that this doctrine of Johnstone's, which to me is as effectual in the explanation of the physiology of menstruation as it is capable of adaptation to the pathology of the uterus and its appendages, will probably lie in obscurity for a century or two, until we have got rid of the cobwebs of German transcendentalism with which the whole of this question has been for the last twenty-five years embittered.

We have given these views about menstruation at great length because Tait firmly believed in them, and was never tired of explaining them to any one who would listen to him.

With regard to the above views of Tait, Johnstone, and in a measure, of Bland Sutton, later observers have not readily accepted them. The whole subject is more or less framed in theory and supposition, and at present we may say that while certain facts are admitted the whole subject is treated in text-books as though it stood on a tentative basis that might be swept away at any moment by a new array of observations and facts; at present, however, the following points are very generally accepted :—

Relation between Menstruation and Ovulation

[By ovulation we understand the rupture of a ripe Graafian follicle with the consequent escape of the ovum.]

(1) Menstruation and ovulation usually occur about the same time.

(2) Menstruation is dependent upon the presence of the ovaries.

(3) Menstruation and ovulation are not always synchronous.

(4) Ordinarily the Fallopian tubes take no part in the menstrual function.

(5) In some cases there is a slight amount of bloody fluid exuded from the tubes at the menstrual period.

(6) The theory that menstruation is regulated by the "menstrual nerve," as advocated by Tait, Johnstone, and Savage, has not been definitely proved.

Leaving menstruation now, we find that Tait alludes to his observation on the effect of the exanthemata on the uterine appendages, and says that the experience of years only confirms his original observations that cases such as scarlet fever and small-pox cause attacks of pelvic peritonitis with involvement of the Fallopian tubes and ovaries. He says: "I have no doubt now that there is a specific form of oöphoritis associated with certain exanthemata, more particularly scarlet fever and small-pox." These observations of Tait have been confirmed inasmuch that recent observers agree that the changes, caused by infectious diseases upon the ovaries, vary in degree and are exactly the same as those produced by them upon other parenchymatous organs. Abscess of the ovary during typhoid fever has been reported by Hirst (1905), and Dirmoser (1904) observed that the uterine appendages were inflamed during typhoid fever. Kelly remarks: "It seems probable that the pelvic organs are more or less affected in almost all cases of infectious diseases, but if the changes are slight they will pass away with recovery, like other changes incidental to the disease; on the other hand, should there be disturbances of a serious character, the foundation for future disease of a subacute or chronic nature will be laid." Penrose says: "It is probable that some of the cases of arrested development in the internal organs of generation, as well as cases of chronic tubal disease and ovarian disease seen during later

life, may be traced to this exanthematous form of endometritis occurring during girlhood."

Tait then goes on to deal with cases of inflammatory diseases of the uterine appendages due to specific infection —that is, the result of gonorrhœa. He says :

> With my increasing experience there is an ever-widening belief in the extent and importance to which this cause is to be laid down as the origin of suffering in women. . . . I am certain that hundreds, if not thousands, of instances are occurring annually in which serious and even fatal mischief is done by gonorrhœal infection, the victims of which are entirely unconscious of its primary infliction.

" In England Lawson Tait was one of the first to insist on the part played by gonorrhœal infection in the diseases of women " (Hellier).[1]

It is little to the credit of gynæcologists that gonorrhœa, as a factor in the causation of pelvic disease, was overlooked. Bernutz and Goupil had in 1857 recognised the fact that gonorrhœa spread from the vagina through the uterus to the Fallopian tubes, and they drew attention to the fact that pelvic peritonitis was very frequent in women with gonorrhœa.

Tait made his first observations on the occurrence of gonorrhœal inflammation of the uterine appendages in 1870 and 1871, and these are alluded to in the Hastings Prize Essay (*British Medical Journal*, 1873). He says :

> As this essay was written in the end of 1871 and the beginning of 1872, and in the hands of the adjudicators for many months, it must be evident that its conclusions were made quite independently of those of Dr. Noeggerath, whose pamphlet was not in this country until 1873, and was never seen by me till 1875. I mention this only because some of my German critics have stated that I have taken my views from Noeggerath without acknowledgment, and this is simply not the fact. They were formed quite independently and have not been altered in any way save by way of extended information.[2]

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[1] " System of Gynæcology," Allbutt, Playfair, Edinburgh, 1906, p. 553.

[2] " Diseases of Women and Abdominal Surgery," p. 371.

In 1872 Noeggerath published in Bonn the observations which he had made in New York, and this revolutionised the view of the medical world regarding the clinical significance of the disease, more especially in reference to the effect of the disease on women. He maintained that gonorrhœa was very intractable to treatment; that it often remained latent for months, or even years, before causing severe complications, and that infection, the result of sexual intercourse, might result after long periods of quiescence. He also pointed out how frequently sterility follows gonorrhœal infection in women.

While Noeggerath suspected the etiological factor of gonorrhœa, he did not discover the micro-organism; that was left to Neisser to do in 1879.

In England, Sinclair, of Manchester, did much to bring the subject before the profession, and he adopted Neisser's germ theory of the disease. Tait acknowledges his contribution, but will have nothing to do with the gonococcus.

Tait in his work was able to confirm many of Noeggerath's views; and among them he drew attention to the condition of latent gonorrhœa in the male. He pointed out how a young man, apparently cured of this disease, would marry, and a few weeks after the marriage his wife would develop the disease, and this might lead to an infection of the uterine appendages.

It remained for Tait to demonstrate to the world what a serious disease gonorrhœa was for the female; this he did when he removed pus tubes, and he says:

> When I first began to show my preparation of pyosalpinx and narrate the cases for which they had been removed, some twelve or fourteen years ago, at our medical societies, there were no expressed doubts as to the facts, for the evidence was incontrovertible; and I was able to show that our old authors had known all about such cases, and had fully described them, but it was on all hands asserted that they were rare. But as my experience went on and my mortality diminished, as, in fact, I was able to give women relief from lifelong suffering at a constantly diminishing price, in the form of lessening risk, I was able to show that these cases instead of being very rare, were extremely numerous. At the International Congress of 1881, the celebrated criticism of

Sir Spencer Wells, " I have seen only one such case in my life ; I suppose they must all go to Birmingham," gave the keynote to professional incredulity. But the time came when the facts were fully confirmed and the awful conclusion forced upon us that gonorrhœa is, as I have said, a terrible and fatal scourge.

Norris, in his work on " Gonorrhœa in Women " (1913), says, as regards Tait's contribution :

No historic sketch on gonorrhœa, however brief, would be complete without a reference to the wonderful skill and genius of Lawson Tait, who performed much of the pioneer work in the surgery of pelvic inflammatory diseases. He recognised the tubal origin of these cases, and the results of the " Tait operation," as salpingo-oophorectomy for adnexitis was called, were so brilliant as to attract general attention throughout the entire surgical world.

The concluding portion of the work, from p. 438 to p. 540, is devoted to Ectopic Pregnancy and Pelvic Hæmatocele, and this portion we have already dealt with in a former chapter.

XXX

The Evolution of Hysteromyomectomy

We propose to sketch the history of abdominal hysterectomy for fibroid tumours of the uterus, tracing the various procedures which, beginning with those early attempts in the middle of the nineteenth century, were continued with but little success until Koeberlé introduced his extra-peritoneal treatment of the stump, and applied his wire-constrictor. He led the way to success, and we shall show how Keith, Hegar, Elder, Tait, Hegar and Pozzi, by introducing modifications of Koeberlé's plan of treatment, gradually brought the extra-peritoneal method of treatment of the stump to comparative safety. Then we shall show how the extra-peritoneal treatment of the stump was swept aside, and after many attempts at an intra-peritoneal technique, the simple procedure of the present day was established.

Péan, Urdy, and Spencer Wells divide the history of abdominal hysterectomy into three periods; we shall add a fourth.

The first period is antecedent to 1843. Before that date surgeons from time to time opened the abdomen with the intention of removing a supposed cyst of the ovary, but on finding that their diagnosis was incorrect, and that they were face to face with a solid tumour of the uterus, they very wisely abandoned the operation, believing that such tumours were beyond the sphere of legitimate surgery. To this period belong the reported cases of Lizars (1825), Dieffenbach (1826), and Nathan Smith, and at a later date those recorded by Atlee, Baker Brown, Cutter, Deane, and Mussey. Fourteen operations of this kind were published during this

period of uncertainty in diagnosis, of which five resulted fatally.[1]

Hegar says: "During this period the removal of pedunculated fibromata of the uterus was the first operation of the kind attempted. Granville's operation (1837)[2] terminated fatally, but Atlee and Lane (1844) successfully extirpated subserous polypi."

The *first period* ends at 1843 when Clay, of Manchester, removed a fibroid tumour by abdominal hysterectomy, with fatal result.

The *second period* begins with Clay's operation in 1843, and extends to 1863, when Clay did his first successful cases, and Koeberlé came into the arena.

In this second period, which Péan calls the stage of essaying and groping, and when ovariotomy had been attempted by many; some surgeons, finding themselves after an error of diagnosis in the presence of a uterine tumour, improvised, as Spencer Wells says, the operation, and removed the tumours through the abdominal incision. Among the operators of this period we may mention Clay (1843), Heath (1843), Parkman (1848), Peaslee (1855), Boyd (1856), Spencer Wells (1859), and Sawyer (1860).

It was during this period that the classical cases of Bellinger (1846), of Burnham (1853), and Kimball (1853) were performed.

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[1] "De la Valeur de l'Hystérotomie dans le Traitment des Tumeurs Fibreuse de l'Utérus," S. Pozzi, Paris, 1875.

[2] Hegar's statement about Granville's operation has been quoted many times. Hegar gives no reference for his statement. Pozzi in his thèse ("De la Valeur de l'Hystérotomie," 1875) gives under 2e Tableau Statistique, 46, "Granville, 21 Mars 1827, Tumeur pédiculée, Mort, à la suite d'une mortification de l'intestine," and quotes Lee, *Med. Chir. Trans.*, xxxiv. p. 14. On looking up Lee's "Clinical Reports," we find the following: "Several years ago an eminent accoucheur of this metropolis made an incision through the abdominal parietes of a young woman who had a movable tumour in the belly, which he considered to be ovarian, and which he thought possible to extirpate, as Mr. Lizars had done in a similar case with success. On laying open the abdomen, a large fibro-cartillaginous tumour presented itself, which was attached to the fundus uteri by a thick peduncle. A ligature was applied round this, and the tumour cut off; but death soon followed, in consequence of gangrene taking place in that portion of the bowel which had come in contact with the cut surface." The surgeon's name is not given, and we do not know how Pozzi and Hegar knew that the operator was Granville, and why their dates are given as 1827 and 1837.

The *third period* is ushered in by the splendid success achieved by Clay in 1863, and followed a few months later by that of Koeberlé.

Soon after Koeberlé introduced his method of extra-peritoneal treatment of the stump, other surgeons took up the operation, and Péan, Hegar, Billroth, Kaltenbach, and Schroeder, on the Continent, did much to advance the technique of the operation; while in England Keith, Bantock, Tait, and Thornton became conspicuous by their successes.

The *fourth period* began when Schroeder (1881) made a systematic attempt to arrive at a trustworthy *intra-peritoneal* treatment of the pedicle. This plan was then modified by some who attempted to remove the uterus completely; and this in turn gave way to the present-day plan of sub-total hysterectomy, which is the operation now universally performed.

Let us now go back and examine some of the details of the early operations so as to appreciate the difficulties that surgeons had to overcome, and let us bear in mind that they had two potent factors which dogged them for years: sepsis and the oversight of a surgical principle, *viz.*, that if you remove a limb, or a tumour, the first essential is to secure the artery that is supplying the limb, or the tumour. Germs they knew nothing of, but their failure to secure the uterine artery as the essential step in the technique is inexcusable, yet this neglect is the factor that governed their want of success for fifty years or more.

Amussat was attracted to the subject of the operative treatment of fibroids of the uterus, because all writers, not only anterior to his time, but even his contemporaries, believed that no fibroid tumour of the uterus—except it was in the polypoid form—could be removed successfully. He therefore determined to attack these tumours by vaginal myomectomy, and in 1842 he reported two successful cases operated on in 1840.[1]

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[1] Amussat, "Memoire sur l'anatomie pathologique des tumeurs fibreuses de l'utérus et sur la possibilité d'extirper ces tumeurs lors qu'elles sont contenues dans les parois de cet organe," Paris, 1842.

Thus we may say that the first deliberate operation for the removal of a fibroid tumour was that of Amussat, in 1840. But Amussat attacked these uterine tumours by the *vaginal* route, and we are not at present concerned with vaginal myomectomy; still Amussat's operations had such undoubted merit that it would be unjust to him not to mention them in the evolution of the technique of hysterectomy.

Charles Clay was the first surgeon to remove a fibroid tumour by abdominal hysterectomy. This is the same Charles Clay, of Manchester, that Tait proclaimed as the man who had established ovariotomy in England.

Clay's first operation was performed on November 17, 1843. He diagnosed the tumour as an ovarian cyst, but when he discovered his mistake he proceeded to perform a supra-vaginal hysteromyomectomy, but the patient died of hæmorrhage a few hours after the operation had been completed.

A few days after Clay's operation, Heath operated on a case which he thought to be an ovarian tumour attached to the uterus. He passed double ligatures through the cervix, below the tumour, by means of an aneurysm needle, and tied them so as to include the broad ligaments; after this supra-vaginal hysterectomy was performed. The tumour was found to be a submucus fibroid, and the patient died from hæmorrhage seventeen hours after the operation.

In 1842 Clay opened the abdomen thinking that he was about to deal with an ovarian tumour; but he found that it was a fibroid of the uterus, and he abandoned the operation. However, in 1844, when he again made the same error, he determined to practise total extirpation of the uterus, and he completed the operation, but unfortunately the patient died on the fifteenth day after the operation had been performed. He says:

> On the twelfth day she was doing so well that every reasonable hope was entertained of her ultimate recovery: a nutritious diet had been given and everything promised well. From the fact of both ovaries and uterus having been extirpated, my interest was doubly excited. On the thirteenth day the nurse put an end to all the flattering prospect

by an accident. On lifting her from the bed to ease the bedding, the patient fell to the floor . . . and she died on the morning of the fifteenth day.

Clay, as the result of his experiences in this case, determined to extirpate the uterus and ovaries if ever a favourable opportunity presented ; but he had to wait until 1858 before the third case appeared, and then he delayed the operation until January 2, 1863.[1] Clay then did his first successful operation for fibroid tumour, performing a supra-vaginal amputation of the uterus.

He says in his report of the case :

I therefore fixed January 2, 1863, for this operation, stipulating that after making an exploratory incision I should be at liberty to forego the extirpation if any peculiar difficulties presented themselves. . . . I made an incision of about eleven inches, when a large uterine tumour came in view . . . though there were no adhesions, it was with the greatest difficulty it could be dislodged from the pelvic cavity. I dissected the broad ligaments from the tumour and, securing them by ligatures, divided them, and continued my progress down to the cervix. Having satisfied myself by passing my finger well round it, I placed a ligature on it just immediately above the plane of the os, consisting of three strands of strong Indian hemp, and then divided the cervix . . . the tumour was removed with very trifling loss of blood ; the integuments were brought together and sewed by six interrupted sutures . . . and the patient placed comfortably in bed in about fifteen minutes from the commencement.

By the eighteenth day all the ligatures had come away and the patient recovered, and left the hospital thirty-five days after the operation.

" Thus," as Noble says, " Clay was the first surgeon to perform hysteromyomectomy,[2] and the first English, or European, surgeon to perform a successful hysteromyomectomy for fibroid tumour." [3]

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[1] Clay in a note to his paper says : " It is necessary to make this statement [that his operation was the first operation in the world where both uterus and ovaries were extirpated through the abdominal wound] as Professor Koeberlé of Strasburg thought that he had performed this operation first on April 20, 1863. My first was on January 16, 1844 ; the second on January 2, 1863."

[2] Unsuccessfully.

[3] " The History of Early Operations for Fibroid Tumours," *Amer. Jour. Obstet.*, August, 1899. We are much indebted to this excellent paper.

It is, however, to America that we must turn if we wish to trace the early operative work on uterine fibroids.

It was W. L. Atlee who performed the first successful abdominal myomectomy in 1844; while the first myomectomy performed in England was not done until ten years later, and then by Eddison (1854).

In 1846 John Bellinger [1] appears to have carried through the first deliberate hysteromyomectomy for a fibroid of the uterus. He says in his description of the operation:

> This enabled me to elevate the tumour and divide the lateral ligaments. They were very vascular and many arteries (the number not distinctly remembered) required tying. Animal ligatures were applied to all of them. The neck of the uterus was cut across about three-quarters of an inch above the os tincæ, and the entire tumour removed. The wound was closed by sutures, plasters, and bandage. Symptoms of peritonitis soon appeared; the inflammation progressed and proved fatal on the fifth day.

Noble remarks: "Bellinger's operation was clearly a hysterectomy for a fibroid tumour of the uterus, and it is equally evident that his operation was undertaken deliberately with the recognition that the tumour was of uterine origin. . . . It is probably correct to consider that he was the first surgeon deliberately to perform hysterectomy."

After Bellinger's operation we come to the one done by Burnham in 1853. This operation was not a deliberate one for a tumour of the uterus, but was undertaken with the idea that the tumour was an ovarian cyst. It appears that, after making the abdominal incision, the patient vomited, and extruded the tumour, which could not be replaced within the abdomen, so Burnham was driven to remove the tumour. He performed a subtotal hysterectomy and tied both uterine arteries. He is thus to be credited with successfully accomplishing what no surgeon had ever done before, that is, he successfully performed hysteromyomectomy, but he did not plan the operation, and he performed it almost as it were by accident, rather than by deliberate plan. Bellinger

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[1] *Southern Journal of Medicine and Pharmacy*, May, 1847, vol. xi. No. 3, p. 241.

seems to have been the first to have conceived the operation, and to have carried it out, but his case had a fatal result; Burnham was driven to perform this operation, and he was successful!

G. Kimball, of Lowell, Mass., *was the first to deliberately perform hysterectomy for fibroid tumour with a successful result.* This operation was done September 1, 1853.[1] Kimball did a supra-vaginal amputation and dropped the stump back.

When we sum up the merits of the work of *Bellinger*, *Burnham*, and *Kimball*, we must give to the latter the chief praise, for he conceived the nature of the tumour that he was dealing with, and he successfully performed the operation and advocated its performance in a formal paper. His merit is greater than Burnham's because he did the operation deliberately, while Burnham did it from compulsion; Kimball advocated the operation upon rational grounds and did not fail to recognise its real nature, while Burnham did not really appreciate the value of the procedure.

In 1853 Washington N. Atlee[2] published his essay upon the surgical treatment of fibroid tumours. He strongly advocated operations on these tumours, but his own successes were largely obtained by attacking the tumours by the vaginal route. Atlee, however, must be given some degree of credit because, as we have mentioned above, he was the first surgeon to successfully perform (1844) myomectomy by abdominal section.

In tracing the history of abdominal hysterectomy, we pass now from the successful cases of Burnham and Kimball in America, to the successful case of Clay already referred to (1864). After these the next surgeon to claim our attention is Koeberlé, who published, in 1864, his historical paper upon hysterectomy, and introduced his method of securing the pedicle by means of the *serre-nœud*, which was invented by Koeberlé for his first operation, performed in April, 1863. Clay had performed his operation in January, 1863.

[1] First "Successful Case of Extirpation of the Uterus" for Fibro-myoma, *Boston Med. and Surg. Journal*, May 3, 1855, p. 249.

[2] Prize Essay, "The Surgical Treatment of Certain Fibrous Tumours of the Uterus," *Trans. Amer. Med. Assoc.*, 1853, p. 559.

Koeberlé's paper [1] is one of the landmarks in the history of hysterectomy because it brought the work, that had been already accomplished in America and England, under the notice of continental surgeons, and it was the means of stimulating the progress of hysterectomy.

Boinet appears to have actually been the first of the continental surgeons to operate on a fibroid, but his case (1858) was not successful; he performed a supra-vaginal hysteromyomectomy.[2]

Koeberlé tries to claim the distinction of being the first operator who had done a deliberate abdominal hysterectomy. He admits the case of Burnham, which was operated on with a false diagnosis, and the two fatal cases of Kimball, but he questions the successful case of Kimball. In the same way he casts aside the case of Clay, by alleging that in Clay's successful abdominal hysterectomy, which antedated his own, the diagnosis of Clay was not entirely certain.

Noble puts the matter very clearly. He says, " A careful study of the cases of Koeberlé and Clay will show that both of them were somewhat doubtful about the diagnosis before operating, and that both of them undertook the operation with a determination to do whatever was best after the abdomen was opened. This makes Koeberlé the second European surgeon deliberately to undertake hysterectomy."

Péan and Urdy, in their monograph on Hystérotomie (Paris, 1873), in referring to Koeberlé, say, " C'est donc à un chirurgien français que revient l'honneur d'avoir le premier pratiqué, en pleine connaissance de cause et de propos déliberé, l'amputation de la matrice."

Pozzi, in his Thèse (1875), also claims that Koeberlé was the first to practise deliberate abdominal hysterectomy; but in his " Traité de Gynécologie " (1905), he says, " Par ces examples, on voit que Koeberlé n'est pas l'auteur de la première hystérectomie abdominale practiquée d'une manière raisonnée et méthodique." [3]

[1] " Documents pour servir à l'Histoire de l'Extirpation des Tumeurs fibreuses de la Matrice par la Méthode sus-pubienne," *Gaz. Méd. de Strasburg*, No. 2, 1864, p. 17.

[2] *Gaz. hebd. Méd. et Chir.*, No. 8, 1873.

[3] This edition (4th) of this splendid work is edited by Jayle, who is probably responsible for the above paragraph.

The publication of Koeberlé's paper was an important event in the history of hysterectomy, for in this paper he introduced his method of securing the pedicle with the *serre-nœud*, and his paper, as Noble remarks, forced upon the attention of European surgeons the work which had been done in America and England, and in this way powerfully stimulated the progress of hysterectomy, by giving an excellent résumé of all that was known upon the subject at that time.

But it is to Péan that we must give the credit of placing the operation on a scientific foundation. He first described a precise method of operation, and operated on diagnosed cases, and on account of definite indications (Hegar). In 1873 Péan had reported nine sections for fibroid tumours of the uterus, with seven recoveries, and these favourable results led to a rapid adoption of the technique advocated by Koeberlé and Péan, whose names will always be honourably associated with the operation of hysterectomy.

Between 1870 and 1880 the number of operations performed for the removal of fibromata of the uterus increased each year. The chief reasons for this increase were that when Koeberlé introduced his metallic loop and ligature tightener for the pedicle, the chief danger in the past, *i.e.* hæmorrhage, was overcome; but the danger from sepsis still remained; this, however, year by year became less, because the teachings of Lister were being applied with zeal on the Continent, indeed, far more than in England.

In the evolution of the methods the first innovation made was the substitution of a clamp for Koeberlé's *serre-nœud*. In the hands of many this was not a success, and when Keith adopted the clamp he became the most successful surgeon of the day when operating on fibroid tumours of the uterus.[1]

As time went on the wire *serre-nœud* and the clamp were replaced by elastic ligatures, first employed by Kleeberg

[1] "For magnitude of operations, perfection of methods, and brilliancy of results, Keith holds a commanding position among all operators."—Greig Smith.

of Odessa, in 1878, and Martin of Berlin.[1] After 1879 Hegar often employed elastic ligatures in perfecting the extra-peritoneal method. These ligatures were so applied that hæmorrhage was checked with certainty, and Hegar further secured closure of the abdominal cavity by stitching the peritoneum around the stump; the latter being made aseptic by the actual cautery and the application of chloride of zinc.

The results obtained by this plan were so much better than anything that had been tried that it appeared that the extra-peritoneal method would be the ultimate plan of operation. But the excellent results which had followed the intra-peritoneal treatment of the pedicle in ovariotomy had stimulated Schröder to try and treat the pedicle by an intra-peritoneal method, which he advocated for the first time at the Cassel congress in 1878, then at the Baden congress in 1879, and finally at the Salsberg congress in 1881.

The next step in the development of the operation was the total extirpation of the uterus, and after that an attempt to remove the myoma and leave the uterus. This was attempted in some cases of interstitial and subserous interstitial growths, and the names associated with this difficult procedure are the names of three masters of technique, August Martin of Berlin, Howard Kelly, and Olshausen of Berlin.

In writing the history of an operation, it was necessary in years gone by, to give statistics in order to show how one method excelled a rival method. Although it has often been shown that statistics of operations are not of great value, yet they will serve to give some approximate idea of the result of operative interference, and so we produce a table here, compiled by Gusserow, giving results up to 1885, to show how fatal the operations for removal of uterine fibroids were up to that time.

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[1] August Martin says: "Schröder mentions Kleeberg as the author of this method of constriction. Kleeberg, however, only drew rubber strings through the tumour in place of the wires of Péan, while I introduced the constriction on the principle of the rubber tube of Esmarch."

Operator.	Date.	Cases.	Recoveries.	Deaths.	Mortality.
Bantock	1883	22	20	2	9·09 per cent.
Tait	1882	30	20	10	33·3 ,,
Wells	1882	40	19	29	52·5 ,,
Thornton	1882	25	16	9	36·0 ,,
Koeberlé	1882	19	9	10	52·0 ,,
Billroth	1882	25	10	15	60·0 ,,
Schröder	1884	100	68	32	32·0 ,,
Hegar and Kaltenbach	1881	12	11	1	8·3 ,,
Savage	1882	9	6	3	33·3 ,,
Thomas	1882	13	7	6	46·1 ,,
Kimball	1883	11	6	5	45·4 ,,
Péan	1881	51	33	18	35·2 ,,
Keith	1883	25	23	2	3·0 ,,
Kaltenbach ..	1883	10	9	1	10·0 ,,
Olshausen	1884	29	20	9	31·03 ,,
A. Martin	1884	60	27	23	38·3 ,,
K. Braun	1884	16	10	6	37·5 ,,
Tauffer	1884	16	12	4	25·0 ,,
Gusserow	1884	20	12	8	40·0 ,,
Total		533	338	185	Average mortality 34·89 per cent.

XXXI

Tait's Contribution to Hysteromyomectomy

We have in the preceding chapter glanced at the evolution of hysteromyomectomy. We have seen that McDowell's success in ovariotomy suggested to other surgeons the possibility of attempting the removal of ovarian cysts, but when, on opening the abdomen, they discovered that they had made an error in diagnosis, and were face to face with a solid or cystic tumour of the uterus, they had not the courage to attack the difficulty and closed the abdomen. But a few were more intrepid than the rest, and attempted what appeared to be, and really was, a very grave operation. If they were fortunate enough to attack a fibroid with a pedicle they sometimes were successful, but if they had to face a large solid tumour of the uterus, and they endeavoured to remove it, the patient died from hæmorrhage or sepsis. But the time came when these difficulties were mastered, and the result was the outcome of good surgery, not good luck. Let us, in this chapter, give in fuller detail some of the factors which led to this consummation, and in doing so we shall bring into this history the influence that Tait exerted on the progress of hysterectomy.

We may start from the time when Koeberlé came on the scene. We do not for a moment under-estimate the work of Burnham and Kimball, but Koeberlé was the surgeon who pointed out a method of removing fibroid tumours which was much safer than anything that had been suggested before this time, because, by his plan, the pedicle was treated by an extra-peritoneal method which lessened the risk of sepsis, and, at the same time, gave the operator good control over the blood-vessels ; sepsis and hæmorrhage being the two dangers of the operation.

Koeberlé's plan of operation is thus described by Thomas :

> His method was to enclose the pedicle of the tumours, that is, the smallest portion of the mass next to the cervix, by a steel wire carried in an instrument called a constrictor, which permitted the gradual tightening of the wire until it firmly encircled the point of constriction. Hæmorrhage was thus prevented, and the pedicle of the tumour held in place after the ablation of the mass above the constricting wire. The abdominal wound was closed, the pedicle being held in the lower angle of the wound by the constrictor.

In some cases Koeberlé, by a single puncture from before backward, passed two metallic ligatures through the uterine mass, and tying one on each side, removed each half of the organ with corresponding ligament (Courty).

Péan, of Paris, soon followed in the footsteps of Koeberlé, and in his work entitled " Hystérotomie," [1] published by himself and his associate Urdy in 1873, he created surprise by his reports of numerous successful cases of fibroid tumours removed by abdominal section. Péan's procedure differed from that of Koeberlé, inasmuch as he always endeavoured to diminish the bulk of the tumour by excising portions of it through the abdominal incision.

He prevented hæmorrhage during this *morcellement* by using large compression forceps, or by encircling the pedicle with a metallic ligature twisted by the knot-tightener (*serre-nœud*) of Cintrat. To prevent the slipping of the pedicle, two long steel pins were passed at right angles through the pedicle above the wire.

Péan's first case was in 1869, and it was the first successful case in Paris. It was a fibro-cystic tumour, and it was not until 1871 that he removed a solid fibroid. Up to 1872 Péan had operated on nine patients, seven of whom recovered.[2]

We might remark here, with regard to nomenclature, that Péan uses the word hysterotomie, its English equivalent

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[1] " Hystérotomie de l'Ablation partielle ou total de l'Utérus par la gastrotomie, étude sur les tumeurs qui peuvent nécessiter cette opération," J. Péan et L. Urdy, Paris, 1873.

[2] We may note here that abdominal hysterotomy was (about 1870) formally condemned by the French Academy of Medicine.

being hysterotomy, which from its etymology means section of the uterus; with the adjective abdominal, it may be applied, as Pozzi remarks, to every operation where uterine tissue is removed after opening the abdomen.

Hysterectomy is the term almost universally employed, and we owe this to Tillaux, who first proposed it in a communication to the Academy in 1889. Subtotal-hysterectomy implies that a portion of the cervix is left behind.

Formerly the Germans were fond of the words "myotomy" or "myomectomy," which implied the removal of a myoma with all, or a part of, the uterus, and this included both hysterotomy for pedicled fibroma, and partial hysterectomy for interstitial fibroma.

Now we imply by myomectomy the enucleation of a myoma, or fibroid tumour, without the sacrifice of any material portion of the uterus; it is thus contrasted with hystero-myomectomy, which is the removal of the uterine body together with the tumours, and with pan-hysteromyomectomy, which is the removal of the entire uterus with its myomata (Kelly).

It was in 1874 that Tait did his first successful operation for the removal of a large fibro-myoma of the fundus uteri by abdominal hysterectomy.

The tumour was removed at a point just above the internal os; recovery was rapid; and the clamp came away on the eighth day. The tumour was a large one, for it weighed eleven pounds.

The report of his case gave rise to some acrimonious correspondence in the *Lancet*, and this we have alluded to in a former chapter.

This was Tait's first abdominal hysterectomy, for in the letter that he contributed to the *Lancet* at this period (the *Lancet*, November 7, 1874) he says, "it is the only one I have had of its kind—that is, the only removal by abdominal section."

In 1877 Tait writes as follows, in his little work on "Diseases of Women":

> For the removal of a uterine tumour by abdominal section, very substantial reasons must be given, because such tumours

have by no means the fatal tendency which ovarian tumours present ; and their removal has, as yet, not been accomplished with the same successful results as have been obtained in ovariotomy. The conditions which justify the removal of such tumours are : uncontrollable hæmorrhage whilst the patient is yet many years from the climacteric, very rapid increase in size, and pressure upon abdominal or pelvic organs of such a kind as to endanger the life of the patient, or to render it no longer endurable.

I have three times performed this operation involving the removal of the whole fundus uteri, the tumours being all purely myomatous, but in only one instance have I had a successful result. . . . In all three cases I clamped the uterus below the tumour, and kept the clamp outside, using in one case the clamp of M. Koeberlé after Péan's method.

In Tait's book on the " Diseases of the Ovaries " (1883), he gives the following figures of a clamp that he had invented and was using. At this period he had adopted the intra-peritoneal method for treating the pedicle in ovarian tumours, but he believed that there were some cases that should be treated by the extra-peritoneal method. He says (p. 284) :

If to the pedicle of an ovarian tumour, unusually thick and short, or in that of a uterine tumour, it is thought desirable to employ a clamp, then it is evident that what is wanted is a form of circular constriction by some means which will completely arrest the hæmorrhage, will not cut the pedicle, and which will allow the wound to be accurately adjusted round the clamp.

To meet all these requirements I have devised a clamp which is here figured . . . (and he remarks) I have used this clamp on thick pedicles in eleven cases with perfect success, *and six of these were uterine myomata.*

In chapter vi. of the 1883 edition of " Diseases of the Ovaries," Tait supplies the following, which we may insert as a contribution to the history of hysterectomy. He says :

In Mr. Spencer Wells' recently published book is given a list of thirty-nine cases of removal of uterine tumours with twenty deaths. This terrible mortality would be more than enough to condemn the operation, and up to a short time ago, in my own practice, the results were nearly as bad, and I did condemn it very strongly. I found, however, from my failures, that it was, as in ovariotomy, the method of dealing with the pedicle that was at fault, and now that I have

altered and amended this, I have obtained results which seem to promise that I shall do almost as well in the removal of uterine tumours as I have done in ovariotomy.

Of the twenty deaths in Mr. Wells' list fifteen occurred with the use of the ligature for the treatment of the pedicle, and only one with the clamp. My experience is quite in harmony with this. I found when I used the ligature, however tightly it was tied, and however carefully I stitched it up like a stump, as advised by Mr. Wells, the stump shrank in a few hours from the escape of serum, the ligatures loosened, and the patient died of hæmorrhage. In only two cases have I been successful with the ligatures, and in both I added the cautery, and did not sew up the stump. Therefore, it need hardly be said that my experience is not in favour of Mr. Wells' recommendations; and it seems to me more than curious to see Mr. Wells deserting the clamp in the very field where its use is promising to be an advance, after he had used it for twenty years in a field where it was a detriment and a hindrance to all progress. . . .

I found that Koeberlé's plan was open to objection because the small size of the wire cut the pedicle and gave rise to secondary hæmorrhage (in two cases), and (in one case) the wire broke some hours after the operation and allowed fatal hæmorrhage to occur. I therefore devised the clamp . . . which has proved completely successful in every case to which I have applied it. In one of these cases the responsibility of performing the operation had been declined by Mr. Spencer Wells, and another came to me from the Samaritan Hospital, where she had been under the care of Mr. Knowsley Thornton.

When acting as assistant to Mr. Lawson Tait in 1891, we frequently saw him perform his extra-peritoneal operation on fibroids of the uterus, and he was very successful. After delivering the tumour he threw round the lower portion, close to the cervix, a temporary rope-compressor, then cut away the tumour, inserted some steel pins, and then placed a wire-constrictor round the pedicle. The abdominal wound was closed, and the stump left projecting outside was kept from slipping back into the peritoneal cavity by the pedicle skewers; the wire constrictor placed beneath the skewers prevented the stump from bleeding. The stump was kept as dry as possible with boracic acid powder or perchloride of iron solution.

This was a barbarous method, but this term would have been resented in that day because such an excellent surgeon

as Greig Smith, speaking at a meeting of the British Gynæcological Society, resented the method being called even *crude*, for he maintained that it was not the duty of surgeons to consider their operations as if they were anatomical exercises; their object was to save human life, and it mattered nothing to them whether they had a sloughing stump or not if by that method they obtained a great saving of life.[1] Tait, speaking at the same meeting of the British Gynæcological Society (1892), on a paper in which Heywood Smith advised the adoption of the *intra-peritoneal* treatment of the stump in abdominal hysterectomy, said:

> With an experience of many hundred cases he solemnly asserted that in the absence of some great discovery—of a kind not as yet even hinted at—the intra-peritoneal method was impossible for general adoption. His experience was that by the extra-peritoneal method the mortality of hysterectomies need not exceed 5 per cent. . . . and short of some altogether fresh light upon the subject, he intended in future to adhere to the extra-peritoneal method.

Now, as a matter of fact, the "fresh light" had already been thrown on the subject, but Tait had not grasped its significance. What this fresh light was we shall tell presently. In the meantime it is necessary to go back ten years and take note of the valiant efforts being made by Schröder and his pupils to bring to greater perfection the technique of the *intra-peritoneal* treatment of the stump.

If we turn over the pages of Schröder's great work, "Handbuch der Krankheiten Weiblichen Geschlechtsorgane," we shall see that about 1880 he had practised with great success the following technique in performing subtotal abdominal hysterectomy. He first of all tied off the ovarian arteries, then tied the round ligaments, then tied off the broad ligament and divided all the structure up to the side of the uterus. Then he placed an elastic band below the tumour so as to constrict the neck of the womb. Then he tied the uterine arteries above this elastic ligature, and having done that he cut away the tumour and sutured the flaps left above

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[1] *Brit. Gyn. Jour.*, vol. viii. p. 53, 1892.

with several layers of sutures. After this the elastic band was released and the stump dropped back into the peritoneal cavity.

This operation in Schröder's hands was attended by fair success; still it was not sufficiently perfect to satisfy his followers, and other plans were tried.

Czerny, Kaltenbach about 1880, and Olshausen in 1884, recommended that the elastic ligature, which was placed round the cervix below the tumour in Schröder's technique, should not be removed, but it should be sutured about the pedicle with silk thread to prevent it slipping when the stump was dropped back. The pedicle, thus ligatured, did not mortify, but its nutrition was scant and it gradually underwent a granulo-fatty degeneration. Sometimes it suppurated and caused trouble until the elastic ligature came away.

But all these methods were unsatisfactory, so Bardenheuer, about 1881, proposed to remove the whole uterus, and he was able to do so six times successfully out of seven operations. Later, in 1889, August Martin, of Berlin, attempted to revive total extirpation. He first of all did a supra-vaginal hysterectomy after applying the elastic ligature. Then an assistant freed the cervix from the vagina, and the surgeon completed the operation from the abdomen.

Zweifel (1889) now came forward with a method which gradually became very successful in his hands. It was, in fact, only an extension of Schröder's old method. He, in the first place, tied off the broad ligament on either side, then passed a temporary elastic ligature round below the tumour, which he then removed, and transfixed the stump by an elaborate series of sutures. Over the stump he sutured the peritoneum, and then removed the elastic ligature, and returned the stump into the peritoneal cavity. Zweifel's plan was the most successful intra-peritoneal method of treating the stump, and he might have brought it to still greater perfection had not the time arrived when the "fresh light," that Tait spoke of, was about to illuminate the whole field of the operation of hysterectomy.

To Stimson, of New York, is given the credit of having first clearly pointed out the right way, for he proposed and

practised the systematic ligature of the ovarian and uterine arteries in their course as the cardinal preliminary to hysterectomy. By this simple improvement in the technique, as Kelly remarks, the dangerous mass ligatures applied to the broad ligaments were done away with, and the risk of sepsis and hæmorrhage greatly reduced.

Others had made a shrewd guess that this was the proper line to adopt, for we find Kaltenbach writing about 1886 as follows: "The recent improvements in intra-peritoneal methods consist of the isolated ligature of the main bundles of vessels, of suturing the surface of amputations, and covering it with peritoneum, and of precautions against infection from the cervical canal." Treves, of London, with the eye of the expert general surgeon, had clearly indicated the proper technique to be followed by the gynæcologist, for at a meeting of the Medical Society of London in 1890, after Meredith had read a paper on the present position of abdominal surgery, Treves, in referring to abdominal hysterectomy for uterine fibroids, said:

> The blood vessels going to the mass were easy to find and to ligature; they were mainly the ovarian, the uterine and the ovarian branch of the vaginal arteries. Having secured them, the ligaments on each side were divided, and a V-shaped section of the cervix was made, leaving a kind of flap on each side. When the tumour was removed in this manner, a large gash was made in the peritoneum, and after this was stitched up there remained a closed seam running across the pelvis. He contrasted an operation such as this, based on ordinary surgical principles, with the use of the serre-nœud, which might be described as a barbarous instrument, to be compared only with the clamp in ovariotomy, and like it to be ultimately abandoned; the results he had obtained so far by the above method were admirable.

Milton, of Cairo, was one of the first to recognise that the ligation of the vessels was the cardinal preliminary step in hysterectomy.

In America, Stimson's views were taken up by Baer, and he introduced an improved technique in 1892; and, based on these new principles, Pryor, in 1894, and Kelly, in 1896, soon brought the technique to perfection, and when Howard Kelly published his magnificent work on "Practical

Gynæcology," the simplicity of his technique for subtotal hysterectomy taught the whole world the plan that is now universally employed in performing hysteromyomectomy for fibroid tumours.

In the last years of Tait's life he often performed intraperitoneal hysterectomy, and was very successful with such operations, but he added nothing original to the technique.

Simple as Kelly's operation is, we have, for the last twenty years, practised an even simpler technique, with the result that almost any fibroid may be removed in from five to seven minutes.

The steps of our operation are as follows :

1. Patient is placed in the Trendelenburg position; surgeon stands on the left-hand side of table. (Kelly always stands on the right-hand side, and consequently the patient's pubes is an obstruction to his manipulation.)

2. Abdominal incision from pubes up to left of umbilicus.

3. Tumour exposed and intestines packed out of sight by large gauze sponges.

4. Corkscrew inserted into tumour and latter pulled up as far as possible.

5. The round ligament of left side clamped by catch forceps close to uterus, and second pair placed one inch more distal on round ligament.

6. Round ligament cut through between forceps.

7. Right index finger of surgeon pushed from behind broad ligament through the opening made where round ligament is divided.

8. Left index finger enters this opening *from the front*, and right finger withdrawn, and the left finger supports broad ligament and ovary while clamp forceps are placed on broad ligament close to uterus, the point of the clamp forceps emerging at the opening through which the left index finger of the surgeon is protruding.

9. A second clamp forceps put on distal to first forceps on broad ligament, and the broad ligament and Fallopian tube divided up to the opening where the left index finger is.

10. The same procedure is carried out on the right side of the uterus.

11. The left index finger is poked down behind the *anterior* layer of the broad ligament, and then it is swept up towards the uterus, and by this motion the peritoneum, which is on the anterior surface of the uterus, is stripped up, and the bladder is separated from the uterus.

12. A pair of scissors cuts the peritoneum across the anterior face of the uterus, and the bladder is pushed down by a piece of gauze. The bladder always refuses to be stripped away at one spot where the utero-vesical ligaments run from the anterior median surface of the uterus to the back of the bladder. It is generally advisable to snip these ligaments through with the scissors.

13. The bladder being now stripped off the anterior face of the uterus, the operator, taking a long pair of forceps, goes to the right side of the cervix, places the forceps on the uterine vessels; the forceps being placed on at right angles to the middle line of the uterus. A second pair is placed distal to the first and the operator cuts between the two forceps, and then cuts across the cervix about the level of the internal os, and continues until he approaches the left side when the left uterine artery is seized, and the tumour, being completely severed from the cervix, is lifted out.

14. The uterine arteries on either side are tied with silk, the cervical canal is cleansed with peroxide of hydrogen, and the mucus membrane is cut away when a cone of tissue is excised from the cervix, and the sides of the crater thus formed are brought together by catgut sutures.

So simple is this operation that one wonders that Tait and others could have gone on for so many years practising the barbarous extra-peritoneal operation; but the secret of its simplicity and ease of execution rests in the employment of the Trendelenburg position. This position of course Tait never adopted, and consequently many operations on the pelvic organs, that are now quite easy, presented in his day very formidable difficulties.[1]

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[1] We employed the above technique on one series of hysterectomies, and during seven years had no deaths. During that time we operated upon one of the oldest known cases on which abdominal hysterectomy had been performed. Bland Sutton alludes to this case in the following list ("System of Operative Surgery," edited by Burghard, 1909):—

Reporter.	Age.	Nature of Operation.	Result.	Reference.
Bland Sutton ..	73	Subtotal for fibroid 28 lbs.	R.	*Trans. Obstet. Soc.*, 1900, xli. 300.
Stewart McKay	70	Subtotal for fibroid 19 lbs.	R.	*Australian Med. Gaz.*, 1907, 14.
Bland Sutton ..	83	Vaginal hyst. for villous	R.	*Trans. Obstet. Soc.*, 1906, xlix. 46.
Malcolm ..	74	Total for fibroids	R.	*Brit. Med. Jour.*, 1907, II. 1571.

XXXII

Antiseptic and Aseptic Surgery

We now come to deal with Tait's attitude towards Lister and the antiseptic system of surgery.

In order that we may do justice to both Lister and Tait, we propose, in the first place, to sketch the evolution of Listerism, which is the most important feature in the history of the development of modern surgery; and then trace Tait's attitude to Lister's work. We shall consider the subject under four headings:

(1) The Evolution of Lister's Antiseptic Principle and Technique;

(2) Tait's attitude to Lister's Theory and Practice;

(3) A Comparison between the Technique of Lister and Tait;

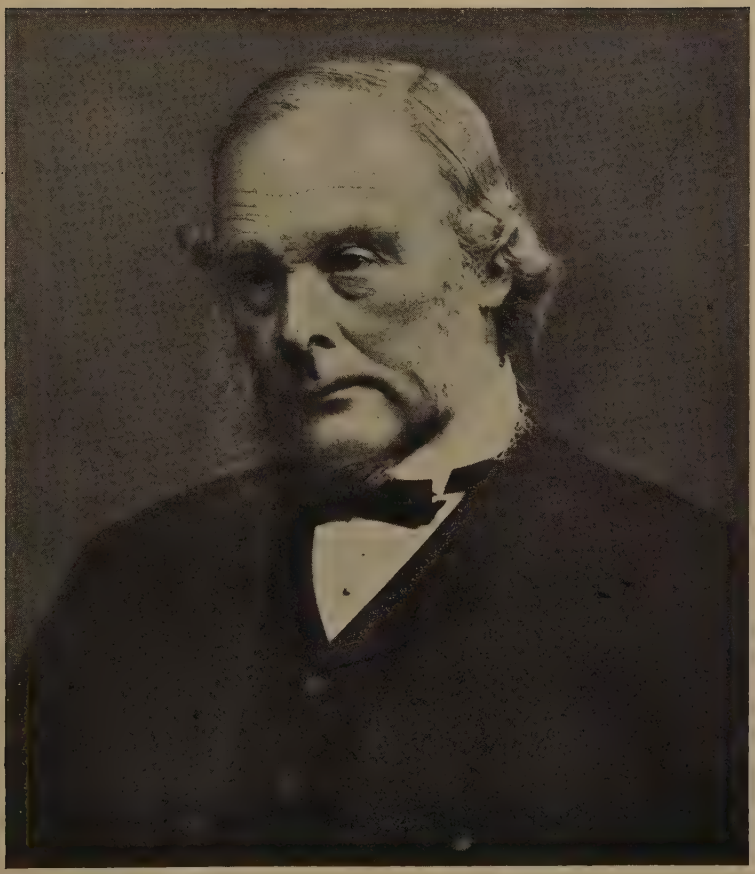
(4) Aseptic Surgery, from Syme to Tait and von Bergmann.

(A) The Evolution of Lister's Antiseptic Principles and Technique

In a former chapter we have alluded to Lister's arrival in Edinburgh, in 1853, to see Syme's work, and to the fact that the visit was prolonged, and ultimately Lister was persuaded to take a house surgency at the Royal Infirmary. Later on he was appointed assistant-surgeon to Syme, and this led to a professorship of surgery at Glasgow in 1860.

During the time Lister remained at Edinburgh he had every opportunity of studying Syme's methods. Tait has told us that Syme was looked upon as the safest surgeon in Europe, and he has also told us that he was the personification of cleanliness. When, therefore, Lister removed to Glasgow he was first amazed, and then appalled, at the

PLATE XXVIII.



LORD LISTER.

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condition of things he encountered at the Royal Infirmary; every case operated on developed sepsis, gangrene, or pyæmia, and the wards were so filthy that he dreaded to operate. Year after year Lister fought to improve matters, and it was during 1865 he first began to try experiments with carbolic acid; then the important hour in the history of surgery came, for in this year his colleague, Dr. Thomas Andersen, the Professor of Chemistry, drew his attention to the writing of Louis Pasteur.

Up to this period Lister does not appear to have known about Pasteur's researches on fermentation and putrefaction; but, after studying the French savant's writings, he surmised that the key to suppuration and wound infection might be due to similar organisms to those that Pasteur had found in the air, and which were apparently the cause of fermentation.

Gradually Lister learned that putrefaction might rightly be regarded as a fermentation, and that the air was the probable cause of the condition, and later on he concluded that it was not the gases in the air, but the dust in the air that was the true cause—because the dust held and conveyed the germs of putrefaction to the wound: hence the air itself was not the cause, but the vehicle that carried the cause. To prevent putrefaction it would be necessary to purify the air before it gained access to the wound. As Pasteur's discoveries had been known for at least some years before this period, it is to Lister's credit that he was the first to grasp their significance as regards wound infection; but "in the field of observation chance only favours the mind which is prepared," as Pasteur has remarked, and so Lister, who was searching for some workable hypothesis to account for the occurrence of suppuration in wounds, showed true genius in hitting on the right cause at the outset of his work.

> Of all Pasteur's discoveries none impressed Lister more than his demonstrations that the organisms, which produce fermentations and putrefactions, are carried in particles of dust floating in the atmosphere, and that these particles of dust can be destroyed by heat, or filtered off by cotton wool,

or intercepted in the finely drawn-out, or tortuous, neck of flasks, through which free ingress and egress of air takes place owing to the diurnal variations in temperature. . . .

Let us now recapitulate the points of special interest to Lister to be found in Pasteur's writings before 1865, and which, it may be imagined, came to him as a light shining in a dark place :

Putrefaction is a species of fermentation. It is caused by the growth of micro-organisms and does not occur independently of their presence.

The micro-organisms that produce fermentation and putrefaction are carried by the air, in the dust that floats in it. They also occur on and in solid and liquid substances.

These micro-organisms can be destroyed by heat and other agencies, or separated from the air by filtration.

Certain recognisable organisms produce definite and distinct fermentative processes.

All of these organisms require oxygen. Some of them flourish only in the presence of free oxygen (aërobic), others only in its absence (an-aërobic). The latter acquire their oxygen from the bodies which, by their growth, they are causing to ferment and putrefy.

Many natural animal and vegetable products having no tendency to ferment or putrefy, even in the presence of oxygen, if collected with proper precautions and kept in sterilised vessels.

Spontaneous generation has never been observed to occur, and this may be regarded as a chimera. (Godlee.)

Thus when Lister began his investigations on suppuration in wounds, in the pestiferous wards of the Glasgow Hospital, in 1865, his simple creed, founded on Pasteur's teaching, was that certain organisms peopled the air and caused putrefaction in wounds. He did not know, and could not have guessed, that not one, but many different organisms, had the power to produce decomposition ; nor did he know that micro-organisms had any other means of access to the living body than through open wounds.

The cause of decomposition being known, and the means of preventing its occurrence in the laboratory having been discovered, it was clear that similar means ought to prove effectual in preventing decomposition in wounds, in which case suppuration would be done away with, and hospital disease would be banished at the same time. (Godlee.)

This, then, was what we may call Lister's Principle, and upon this principle his antiseptic system was based; and we shall have occasion to show that those surgeons, Tait included, who did not grasp, or understand, or admit Lister's Principle, quite failed when they came to practise antiseptic surgery; and because they failed to grasp the *principle*, they abused, ridiculed, and rejected Lister's teaching, quoting their own results and failures as arguments against Lister's routine.

Now, if the above was Lister's *Principle*, what did he mean by his *Antiseptic System?* His own words will explain:

> To prevent the occurrence of suppuration, with all its attendant risk, was an object manifestly desirable; but till lately apparently unattainable, since it seemed hopeless to attempt to exclude the oxygen, which was universally regarded as the agent by which putrefaction was effected. But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended, not on the oxygen or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding the air, by applying as a dressing, some material capable of destroying the life of the floating particles. Upon this principle I have based a practice of which I will now attempt to give a short account.[1]

Thus Lister at the outset tried to prevent the occurrence of putrefaction in wounds; he did not hope to master putrefaction already established in a wound. His endeavour was to try to introduce a new kind of surgery, whose outstanding feature was to eliminate decomposition; his aim was prevention, not a struggle after the infection has taken place. And, as Godlee well remarks, if he had from the first adopted the word *aseptic*, which appears more closely to connote his principle than the word antiseptic, the misunderstandings of later years would never have arisen.

Lister naturally paid most attention to the air as the source of infection, but he soon recognised that, if called

[1] "Collected Papers," vol. ii. p. 37.

upon to deal with a wound, the result of a railway or machinery accident, the wound would be infected by the foreign bodies that have entered the broken surface, and, therefore, he must endeavour to destroy these germs already in the wound before attending to the air that was supposed to contain other germs ready to enter the wound.

In the laboratory, heat, filtration, or chemical bodies could all be employed in the destruction of germs infecting any substance; but in the human body chemical antiseptics appeared the only practical means, and accordingly Lister picked out for use his first chemical antiseptic, and this was carbolic acid.

Lister's initial effort at the application of an antiseptic to a wound was in a case of compound fracture. His colleague, Dr. Thomas Andersen, supplied him with some crude acid, and he soaked a piece of lint in the undiluted acid, and having squeezed out all the clotted blood he introduced the piece of carbolic lint into the wound and passed it freely in all directions in order to destroy any germs introduced at the time of the accident, or which had entered from the skin or the air, or from any body in contact, since the time of the accident. Then a piece of lint, soaked in acid, was applied to the wound just large enough to overlap it in every direction, and this was covered by a piece of block tin to prevent the evaporation of the antiseptic. This dressing was fixed in position by straps of adhesive plaister, and some absorbent material was packed round between the limb and the splints, for the purpose of soaking up any blood or drainage that escaped from the wound.

The metallic plate during the days following was removed from time to time, and some more antiseptic was painted on to the crust of blood and acid that had formed at the edge of the dressings.

Lister early recognised the fact that a successful dressing would have to be mild and unirritating, and, at the same time, that it must contain a store of the antiseptic sufficiently potent, and that the antiseptic should be in such a form as to be potent and not easily removed from the dressing.

His idea was that the dressing must contain sufficient antiseptic to render any germs from the air impotent, and at the same time to act upon the discharges from the wound, and render them incapable of supporting any life, when they gained the outer border of the dressing and then came under the influence of the air,

When we pause to think, we must consider that Lister was lucky in striking on carbolic acid, and still more lucky in using it undiluted. To have swabbed his wounds in connection with compound fracture with a mild antiseptic would have meant failure, but in using pure carbolic acid he struck on one of the most powerful germicides known to us even at the present day. But naturally to use such a potent remedy meant that, while he got a most powerful disinfectant, he at the same time employed an agent that not only destroyed his tissues, but caused intense irritation to the skin and the wound edges.

Experience taught him these defects, and so he sought to dilute the acid. The crude acid he found to be almost insoluble in water, but on obtaining a purer form of the acid, he found it soluble in twenty parts of water; so the familiar five per cent. solution came into being.

At the same time Lister made the discovery that the pure acid was very soluble in oil. Water holds carbolic acid but feebly and yields it up readily, but on the other hand oil holds it firmly; for this reason a strong oily solution has a milder action on the tissues than a dilute watery solution.

Lister, after many experiences, came to the conclusion that if he dressed a wound, from which discharges were expected, as the discharges passed under the oily dressings they took from the dressings enough of the carbolic to make the discharges antiseptic in themselves. Thus an oily dressing was a reservoir of antiseptic which was capable of making the discharges actual streams of antiseptic flowing away from the wound.

But Lister did not find things work out quite as easily as he expected, so he had to change his oily dressings and adopt a putty dressing. This consisted of carbonate of

lime and a solution of carbolic acid in linseed oil (1 in 4). This putty was spread a quarter inch thick on block tin or tin foil; it did not irritate the skin and the living tissues, and as there was no limit to the size of the poultice-like dressings, the discharges could be made to travel as far as desired before they could be infected by the germs in the air.

In the spring of 1866 Lister was able to observe one of his first successful cases of compound fracture of a leg without decomposition in the wound, and in a letter to his father he said: "Well it is now eight days since the accident, and the patient has been going on exactly as if there were no external wound, that is as if the fracture were a simple one. . . . Thus a most dangerous accident seems to have been entirely deprived of its dangerous element."

This case may truly be claimed as the foundation upon which modern surgical work was built, for Lister now saw that he was on the right path, and his successful practice started with confidence from this period. It is quite true that much that he put forth was not *essential*, though he thought it was, and his alterations and withdrawals proved to be the weak points that his adversaries were not slow to attack; but we must give him this credit, that whatever he did was done after serious thought and constant and painstaking experiment, and if what he had tried was found wanting, nevertheless to him, if not to others, it was knowledge, for if it was not necessary it could be discarded, and if it proved to be good and useful it was then an advance.

We must now glance at Lister's practice in regard to lotions applied to wound surfaces, to his use of the spray, and to his long researches on dressings. All of these were at one time or another the subject of bitter attacks from his opponents, and, as the years passed, his antiseptic lotions were abandoned, his spray discarded, while only the most simple of his dressings survived and became universally used.

Lister's primary method of purifying his wounds were altered from time to time. He did not take long to discover that the employment of crude undiluted carbolic acid was

a very potent remedy, and superficial sloughing was the invariable sequence to its application to a wound, so he sought to modify its intense action by employing an oily solution of the acid. But he was not satisfied, so he tried a watery solution of the acid—water holds carbolic acid but feebly, therefore it yields it up readily to substances for which it had a greater attraction. Lister, therefore, employed the watery solution of carbolic acid to cleanse the wound, but still retained the oily dressing to disinfect the discharges.

His next step was to do away with the carbolic putty dressing mentioned above, and in its place he substituted the lac plaister, which was used in the same way as the putty. A piece of suitable size was placed over the wound and its edges were gummed down with plaister, and this mode of dressing held sway until 1870, when Lister discarded dressings intentionally devised for the purpose of not absorbing discharges, in favour of those which absorbed them freely—gauzes.

Thus we come to the gauze dressings, but before we consider their evolution we must mention *the spray*.

Still obsessed by the idea that the air was the chief medium of infection, Lister, after moving to Edinburgh in 1870 to take up his duties as Professor of Clinical Surgery, began to use the spray: his object being to render the air about the wound area innocuous.

> In order to understand why the spray was introduced, it must be borne in mind that Pasteur's teaching strongly corroborated the time-hallowed belief that the air was the principal agent by which wounds were contaminated; and that at this time little had been done in the way of differentiating one kind of bacteria from another, much less in distinguishing those which were afterwards called pathogenic, that is disease-producing, from those which are harmless. (Godlee.)

Lister continued to use the spray, and while it gave a sense of security, it was regarded on all sides as not an unmixed blessing. It sometimes affected the surgeon, at other times it affected the patient, either by irritating

the lungs or by poisoning the wound, and after a long period Lister had to admit that it had done little good, and at the International Medical Congress in Berlin in 1870, he said, as regards the action of the carbolic in the spray, "that it is physically impossible that the microbes in such dust can have been in any way whatever affected by their momentary presence in the spray."

But while Lister admitted that the spray could have had no good action on the germ in the air, he yet maintained "its value as a diffuse and perpetual irrigator, maintaining purity of the surgeon's hands and their vicinity as an unconscious caretaker. But if, beside the spray, we give up all washing and irrigation of the wound, our vigilance must be redoubled." [1]

And now we come to gauze dressings. When Lister substituted gauze dressing for lac plaister, it made no radical change, but it made the antiseptic treatment much simpler for those who tried to follow out their master's instruction, and it was appreciated by patients, who soon recognised the comfort of the new dressings.

This step in Lister's antiseptic treatment is one that his adversaries soon hailed with delight, for they thought that they had laughed him away from his "putty treatment," but they would not, or could not, see that Lister's ideas had not changed, for the putty and the various non-absorbent plaisters had been used so that they might act on the discharges from the wound, as they passed along to the outer border of the dressing; the gauze, impregnated with antiseptics, would now do the work of the putty and the plaister.

The first modification by Lister in his dressing was to adopt oakum, because each fibre of it was "imbued with an insoluble vehicle of the antiseptic," and so if a discharge did soak through, it would not decompose.

But Lister was soon dissatisfied with oakum, but he set himself to apply what he called "the oakum principle, whose aim was to devise a porous dressing, every part of which was capable of holding carbolic acid firmly enough

[1] "Collected Papers," vol. ii. p. 336.

to battle with the discharge from the wound for at least twenty-four hours after its application.

For this three things were necessary :

(1) A convenient " vehicle " for the antiseptic ;

(2) A suitable fabric to hold it ;

(3) Even distribution of the vehicle among the fibre of the fabric.

At first Lister chose resin diluted with paraffin ; for the fabric he chose a muslin gauze ; lastly, the chemical that was to be distributed among the fibres of the fabric had to be found.

Months passed, and years passed, and Lister worked away and experimented, and at intervals gave to the world the carbolic acid gauze, the sublimate gauze, sal alembroth and the double cyanide gauze, and the boracic gauze.

In the preparation and use of all these gauzes, Lister had but one idea and principle, that " all we require is that the dressing shall be able to prevent the development of organisms from without into the discharges with which the dressings may be soaked." [1]

(B) Tait's Attitude to Lister's Theory and Practice

We have in the preceding section sketched the evolution of Lister's antiseptic principles. We now come to trace Tait's attitude towards Listerism.

Tait left Edinburgh to practise at Wakefield in 1867, the year in which Lister published his first paper on the Antiseptic System.[2]

Tait, as we have already mentioned, did his first ovariotomy at Wakefield in 1868, in spite of the fact that he says he left Edinburgh with one final resolve, *that he would not willingly open the abdomen.* From this time on to 1878, he did 50 ovariotomies, and had to admit to the appalling death rate of 38 per cent. Spencer Wells' death

[1] " Collected Papers," vol. ii. p. 296.

[2] *Brit. Med. Jour.*, 1867, vol. ii. p. 246.

rate was the standard of comparison, and as his was only 25 per cent., this left Tait 13 per cent. for which to apologise. He had, during this series, used the carbolic spray, and had followed the canons of Listerian surgery.

Naturally to a man of his temperament this excessive death rate was most repugnant, and he said that he was not satisfied as to the efficacy of the Listerian precautions. In his work on the "Diseases of Women," published in 1877, he gives his first *growl* as regards the "antiseptic treatment." He says:

> If the germ theory be true then the marvel is that any of us are alive, still more that any operation succeeds. Something more than the germs must be looked after, and that is the nidus upon which they may be engrafted. If the stories about the much-valued antiseptic treatment which we hear, *but which are not published*, be true, abdominal section under the carbolic spray is by no means either a simple or a safe proceeding; and when its advocates can perform fifty ovariotomies aided by its hindrances, and have *only six* deaths, then will be the time to listen to them.[1]

Tait worked on from 1878 to 1880 at ovariotomy; while expressing his doubts as to the necessity of the details which were summed up under the term "Listerism," he yet seized on the main idea and proclaimed his belief in "scrupulous attention to cleanliness of every kind and in all directions." He completed his second series of 50 ovariotomies, and this time he had only 3 deaths, as against 19 in his previous 50. He thereupon read a paper before the *Royal Medical and Chirurgical Society* on "The Antiseptic Theory, tested by the Statistics of Ovariotomy,"[2] in which he endeavoured to show that, in his practice, the adoption of antiseptic precautions had not apparently influenced the results obtained in ovariotomy. For while he admitted the truth of the germ theory of putrefaction, he maintained that the practice of antiseptic precautions delayed the healing of wounds, and was accompanied by more constitutional disturbances than careful methods

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[1] Lawson Tait, "Diseases of Women," p. 285, 1877.
[2] The *Lancet*, February 14, 1880.

followed without the details of Listerism. None of the other speakers at the meeting endorsed his views, and they included Spencer Wells, Mathews Duncan, Knowsley Thornton, MacCormac, and Thomas Smith.

During the next year (1881) the *International Medical Congress* met in London, and Tait, in his paper on " Recent Advances in Abdominal Surgery," took occasion to say, in the review of the cases which formed the subject-matter of his very wonderful paper:

> " We have here, therefore, a collection of forty-four operations which would have been regarded five years ago, not only as most serious, but almost wholly indefensible and impracticable; and yet amongst them all, there has been but one solitary death, and in not a single instance have the secondary results been anything but entirely satisfactory. In very few of the cases was the Listerian method employed, and in those in which it was, it seemed to do more harm than good. In my own practice it is entirely discontinued, and I am pleased to learn that the experience I have had of it, and the predictions I ventured upon two years ago concerning it, have been completely substantiated by so great an authority as Dr. Keith, who tells me he has given it up because he has had two deaths from carbolic acid poisoning from it, and that he had now performed forty-six operations for the removal of large ovarian tumours, without Listerism, and with only one death. Since I gave it up, I have performed thirty-one consecutive completed operations without the use of any carbolic acid, and I have not lost a patient.[1]

A few years later, in 1883, when Tait published the fourth edition of his work " On the Pathology and Treatment of the Diseases of the Ovaries," he devoted several pages to a consideration of " Antiseptic Precautions," from which we shall proceed to quote; and from this period on until his death he maintained a marked antagonism to Lister and the theory that germs were the cause of disease.

Let us now briefly examine Tait's objections to Lister, and the germ-theory.

Tait did not deny that putrefaction arose from the presence of minute living organisms in the air;

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[1] " Trans. Internat. Med. Congress," 1881, vol. ii. p. 231.

further, that so far as we know, no phenomena of putrefaction do occur without the admission of these so-called germs to the substance putrefying; that the putrefactive process depends entirely upon them, and the organisms to which they give rise, and that the origin of such organisms within the putrefying fluid, independently of sowing of the seed in the fluid, though by no means to be regarded as an impossibility, is not yet proved as an actual occurrence. . . . For my present purpose, therefore, it is enough for me to observe, as I do most fully, that the germ theory had been completely substantiated, and that no known process of putrefaction does occur save by the admission of resting spores, or swarm spores, of some of the many minute living organisms which are invariably associated with putrefactive changes. But concerning this there is another constant position associated with this phenomena. The materials upon which the experiments have been made, of infinite variety of kind and constitution, have all been dead, and no one had yet pretended that, by the admission of germs to living matter, he has produced the phenomena of the putrefactive changes which constantly result in matter which is dead. . . . It will, therefore, be seen that the application of the facts of the germ theory of putrefaction to the phenomena of diseases of living tissue is met at once by an overwhelming difficulty, to the removal of which none of the adapters, so far as I have seen, have as yet applied themselves. Granted that the same germs which would inevitably produce putrefaction in a dead infusion of beef are constantly admitted to wounds, there is not the slightest particle of evidence that they do produce any change whatever upon living tissues, still less is there any evidence that the changes which occur in the numerous varieties of what we call blood-poisoning, even when they are of an undoubtedly local origin, have the slightest analogy to those seen in a putrefying dead infusion. The mere presence of bacteria in the fluids of wounds, or in fluids enclosed in cavities, whilst offering many difficulties to the adapters of the germ theory, prove nothing for the position until they have shown that these organisms ever do occur in fluids or tissues which are truly living.

We do not propose to quote Tait's subsequent views as regards the influence of germs on the wound, because year after year he repeated what we have already given above: he did not deny that germs existed; he did not deny that they were found in operation wounds; but he persistently maintained that the surfaces of an operation wound were composed of live tissues, while all the experiments to support the arguments of the phenomena of germ development and

growth had been tried on dead tissue, and that no one had shown that germs behaved in the same way on living tissues as on dead matter. He maintained that it had not been shown that germs were the cause of inflammation in live tissues, he said that he believed that germs were the result, not the cause, though he was ready to admit that germs might bring about specific change in any tissue, breaking it up and destroying it.

One may here remark that Tait did not stand alone in his belief. Billroth, the famous surgeon of Vienna, the teacher of Czerny, Mikulicz, and Wolfler, had devoted several years (1867–73) to the study of micro-organisms, and he published his results, but did not admit that micro-organisms were the cause of decomposition, and he stated that he did not believe the antiseptic system stood upon a solid foundation.

We have now pointed out, in the preceding section, the *principle* that Lister based his practice on; we have shown that Tait practised Listerism to the best of his ability [1] in his first series of ovariotomies, and his results were very bad; but at this period it dawned upon him that his results were not due to the observance of Listerian precautions, but to his method of *treating the pedicle.* The moment he gave up the treatment of the pedicle of ovarian cysts by the *clamp* method, and used Keith's cautery method and the *short ligatures*, from that moment his results improved.

During his first fifty operations he had no doubt honestly followed some of Lister's directions, as regards the antiseptic method; but as the antiseptic principle was more often than enough not properly understood, unless the surgeon had actually attended at Lister's clinic, to hear the principle expounded, and to see the practice, it is not unreasonable to suppose that Tait, in common with the majority of the London and the provincial surgeons, had not been fully

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[1] Tait in 1880, in a letter to the *Lancet*, says: "In my experience it was an almost uniform result that wounds dressed after Lister's plan suppurated, and in many cases they sloughed; and though none of the patients died, many of the recoveries were anxious and protracted. Some of my critics said this was due to my imperfect method of using the dressings. My answer was that if the proper use of the dressings was above my intelligence, it was useless for general application."—The *Lancet*, April 24, 1880.

seized with the principle, and therefore carried out the details imperfectly, and so would "deviate from the proper course in some apparently trivial, but essential, detail, and then, ignorant of his own mistake, would attribute the bad result to imperfection of the method." [1]

As Tait was a man of the greatest intelligence, but very obstinate, one can easily imagine that he would not be content to follow any other man's dictates for long. Some one thing would be substituted for some so-called *essential detail*, and then some other detail would be altered, and so in time Tait would discover that he could do an operation without using the carbolic spray; then he would find that the incisions need not be irrigated by carbolic lotions, and that his hands need not be bathed in antiseptic fluids, and so he would get rid of Lister's essential details one by one, and find that they were in a great measure superfluous. Then he turned on Lister and waged war against him; simply because he found he could do a section without the aid of carbolic lotion and the spray!

Here, then, is where Tait was wrong; he refused to admit Lister's *principle* because he refused to accept the microbe as the cause of septic trouble in wounds. We know now that he was wrong, and we know also that the aseptic system, that Tait did so much to bring into existence by his own practice, was only made possible by Lister's work. Tait's care in the preparation of his instruments, ligatures, and everything in connection with his operations, plainly showed that he knew that dirt meant disaster, and whether he admitted it openly or not, he knew that there was a something, call that something germ, or dirt, or by any other name, that once introduced into the wound meant disaster, and accordingly he boiled his instruments, and he boiled his silk ligatures, and in doing that he used the most powerful germicidal agent at his command; and yet he could not see, nor would he admit, that in so preparing his instruments and his ligatures he was obtaining by an antiseptic, *i.e. heat*, an ideal

[1] Lister's "Collected Papers," vol. ii. p. 158.

asepsis, which Lister had pointed out was the aim of all his teaching, and all his technique.

Tait was too pig-headed to give in, and we often think of what Olshausen, the famous gynæcologist of Berlin, said about him, "I am afraid that Mr. Tait seems to identify the notion of antisepsis with that of the use of carbolic acid." And so it was with the majority of English surgeons; they failed to comprehend Lister's principle, and sneeringly regarded antiseptic surgery as a surgery whose chief feature was the employment of carbolic acid lotion and putty.

We may, before concluding this subject, draw attention to the fact that Lister himself was at one period not keen on antiseptic details being employed during the performance of ovariotomy, and the honest position that he took up only served to make his opponents, headed by Tait, more sure that there was nothing in Listerism that the abdominal surgeon need bother about.

Lister expressed his views at the *International Medical Congress*, held in London in 1881. Here are his words:

> To those of you who know from personal experience what antiseptic measures can do for surgery, it may well seem strange to find some who have taken part in these discussions still assigning them a secondary place in the treatment of wounds. The explanation of the fact is, I believe, to be found chiefly in the success—nay, the brilliant success—sometimes obtained without the use of any antiseptic means whatever. This has been conspicuously the case with abdominal surgery, and especially with ovariotomy. Ovariotomy, indeed, has sometimes been spoken of as a touchstone of the efficacy of the antiseptic treatment. The success which has attended antiseptic ovariotomy has been regarded as a signal proof of the truth of the antiseptic principle. Such, however, has never been my own view. Mr. Spencer Wells and Dr. Thomas Keith obtained results which astonished the world before strict antiseptic treatment was thought of;[1] and when, several years ago, Dr. Keith expressed to me an intention of performing ovariotomy antiseptically, I strongly dissuaded him from his purpose. I knew his already brilliant success; I felt that our spray apparatus was as yet inadequate for the production of a cloud sufficiently large to cover the whole

[1] This remark by Lister was true as regards Keith, but was not true as regards Wells, whose mortality was shocking.

field of operation, and sufficiently fine to avoid needless irritation, and I was also aware that such operations are often both very protracted and very anxious, when in proportion to the duration and the anxiety of an operation is the chance of the neglect of some apparently trivial, yet important, element in the procedure. . . . At the same time I believed that the day would come when strict antiseptic treatment would prove valuable in ovariotomy.

(C) A Comparison between the Technique of Lister and Tait

In the preceding sections we have sketched the evolution of Listerism, and we have given Tait's views on germs.

In spite of the antagonistic views held by Lister and Tait, the fact remains that each obtained results in surgery which approached practical perfection. In this section we propose to give the technique that was followed by each, during the course of an operation, and then ask the question : *Viewed in the light of our present-day knowledge, what is our verdict on their methods ?*

We shall suppose that an abdominal section is performed by Lister, and one by Tait ; let us see wherein their methods agreed and differed.

(1) *The Preparation of the Skin.*

(a) *Lister.* The seat of the operation was not specially cleansed.[1] " It is not needful," he said, " to apply an antiseptic lotion for hours together as is sometimes done ; a few minutes' action of the 1 in 20 carbolic lotion is really sufficient ; while its long-continued operation sometimes produces troublesome irritation." This simple preparation was often performed whilst the chloroform—it was always chloroform, not ether—was being administered. Lister declined the use of soap and water, or anything that would remove the greasy matter of the skin, because " carbolic acid has a powerful affinity for the epidermis, penetrating deeply into its substance, and it mingles with the fatty materials in any proportion."

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[1] We have taken most of these details about Lister from Godlee's " Lord Lister," p. 460.

(b) *Tait.* The skin was well washed with soap and hot water before the patient was placed on the table. Tait used no antiseptic fluid to prepare the skin. If the patient was a poor dirty creature he directed that turpentine should be used as an aid in cleansing the skin.

(2) *Preparation of Instruments, Sponges, and Ligatures.*

(a) *Lister.* Before the operation Lister arranged his instruments, which had been carefully washed, but not specially sterilised after the last operation, in a tray of 1 to 20 carbolic lotion, the strength of which was slightly mitigated just before the operation began by adding a small quantity of water; almost invariably the instruments lay in the carbolic acid lotion for twenty or thirty minutes before they were actually used.

The sponges were placed in a 1 to 40 lotion. They had been prepared in what he called "a somewhat rough and ready way."

> I put the sponges after an operation into a tank of water, and let them putrefy there. The fibrine, which clings among the pores of the sponges, becomes liquefied by putrefaction. They can then be washed thoroughly clean of their fibrine, and the washing is continued until they no longer give a red colour to the water. They are then put into 1 in 20 carbolic acid lotion and kept there.[1]

The ligatures were chiefly of sulpho-chromic catgut, brought dry to the operation and then steeped for about half an hour in 1 to 20 carbolic lotion.

(b) *Tait.* Tait prepared his instruments by boiling them in water to which washing soda was added. They were then removed, dried with a clean but not a sterilised towel, and placed for the operation in small sterilised trays and covered by hot water.

His sponges were prepared with elaborate care, for many years by his wife, and afterwards by the matron of his private hospital.

These sponges, when new, were first of all treated with a solution of washing soda or ammonia, and afterwards

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[1] "Collected Papers," vol. ii. p. 353.

well washed; then placed for a week in a five per cent. solution of carbolic. Tait attended to the preparation of his sponges for years, and at one time placed them in a solution of ordinary washing soda, about a pound of soda to the dozen sponges; after being washed in this solution for twenty-four hours they were taken out, and washed with boiled water and dried.

Tait always employed silk for his ligatures; the silk was boiled in water and wound on a broad piece of glass, then dried; just before being used it was thrown into hot water to soften it so that it could be more easily tied.

(3) *Preparation of the Hands.*

(a) *Lister* washed his hands in 1 to 20 carbolic lotion, or in what he called "the strong lotion" (1 to 20 carbolic acid with 1 to 500 sublimate lotion).

(b) *Tait* washed his hands in hot water with soap and nailbrush, but never employed an antiseptic lotion of any kind.

(4) *Dress at the Operation.*

(a) *Lister* removed his coat, turned up his sleeves, and pinned an ordinary unsterilised huckaback towel over his waistcoat. He wore no mask and no gloves. Silence was ordered while the wound was made and the operation performed.

(b) *Tait* removed his coat, put on a large mackintosh apron, but used neither mask nor gloves. No one ever spoke at the operation; Tait himself never spoke until everything was finished.

(5) *The Operation.*

(a) *Lister* arranged towels wrung out of 1 to 20 carbolic round the seat of operation, and during the course of the operation he frequently dipped his hands into 1 to 40 carbolic lotion, and his sponges, after being washed in cold and hot water, were soaked in carbolic lotion 1 to 40, and though wrung out before being used, a small amount of carbolic acid was constantly conveyed to the wound. In addition to this the carbolic steam-spray played on the surgeon's hands and the wound during the course of the operation; and finally, as a safeguard against any contamination, due to the carelessness of assistants or nurses,

he used to wash the wound with 1 to 40 carbolic lotion before sewing it up.

(b) *Tait*, during the course of the operation, frequently washed his hands in warm water, and his sponges were washed in hot water, but no antiseptic fluids were used. If he wished to wash his wound, or to wash out the peritoneal cavity, he used warm water that had been boiled and had cooled.

(6) *Sutures to close the Wound : Dressings.*

(a) *Lister* used sutures of silver wire, silkworm gut, or sometimes fine silk or catgut ; they were all rendered sterile by means of carbolic acid.

The dressing in later years was a thick piece of cyanide gauze covered by a mass of salicylic wool. The gauze next the wound was wrung out of 1 in 40 carbolic lotion.

(b) *Tait* used sutures of boiled silk, and his dressings were simple gamgee gauze. He often covered the sutured wound with dry boracic acid.

Having now stated the different methods followed by Lister and Tait, it behoves us to examine each step and endeavour to decide, in the light of our present-day knowledge, the efficiency of the individual steps of each surgeon's technique.

1. *The Preparation of the Skin.*

The first point that we are called upon to consider is : Should the skin area be washed as a step in the preparation for an operation ? Lister said that if one was called upon to operate under circumstances where there were no chemical antiseptics at the surgeon's disposal, " that thorough cleanliness in the ordinary sense, by the free use of soap and water, must be practised for the hands of the surgeon and his assistants, and for the skin of the part operated on." [1] If, however, the surgeon was able to obtain carbolic, Lister laid down the rule that soap and water should not be used in the preliminary preparation of the skin ; his words are : " For my own part I do not ever use soap or water. I

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[1] *Brit. Med. Journ.*, 1893, vol. i. pp. 161, 277, 337.

trust to the carbolic acid." He gives his reason in another paper: [1]

> As an instance of trouble misapplied in this matter, may be mentioned preliminary washing with soap and water. If carbolic acid is the disinfectant used such washing is not only wholly unnecessary, but is, I believe, positively injurious; as it must tend to check the penetration of the germicide into the substance of the epidermis, by saturating it with water for which carbolic acid has so little affinity. That this practice is superfluous is, I venture to think, proved by my experience, as I never in any case adopt it.

Tait, on the other hand, always had the skin washed with soap and water, and if the patient was one of the lower classes and had been unaccustomed to the bath habit—a habit which appeared to be almost unknown among the lower classes in England—he always had the skin prepared by a free use of spirits of turpentine, followed by soap and water.

What are we to say about these methods for the preparation of the skin?

The first remark that one has to make is that, no matter how inferior these methods may be considered *now*, they gave satisfactory results then. Whatever may be said about Tait's veracity, no one has ever doubted Lister's word, and, as regards "stitch abscesses, Lister used to say that he never had them,[2] and Godlee remarks, "but it is only fair to add that few of his most faithful followers can boast of being so uniformly successful in avoiding them." We may take it, then, that the skin preparation as used by Lister was a satisfactory one. Any one who has studied Godlee's "Lord Lister" will at once admit this, because Lister was so painstaking and so absolutely honest that if he found that any part of his technique was defective he spent days, weeks, months, or even years, to get at the secret of his failure.

Now there is evidence, from Lister's own practice, that he was satisfied that an operation could be carried out

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[1] *Brit. Med. Jour.*, 1908, vol. i. p. 1557.
[2] Godlee, "Lord Lister," p. 469.

according to the *aseptic* principles with success. In his "Common-place Book," dated November 6, 1883, he describes an "Antiseptic Operation without Antiseptic Contact"; he says that he did the operation "with clean but not antiseptic instruments and sponges." There was no drainage tube:

> The dressing was a piece of moist aseptic gauze, eight layers thick covered by a substantial piece of dry salicylic cotton wool. After five days the dressing was removed, and it does not surprise us now to learn that the wound healed by first intention, though, with his usual accuracy and honesty, he described minute rosy areas round each stitch, and one of the same size at the upper end of the wound, all of which had disappeared at the time of the second dressing, five days later. (Godlee.)

With regard to Tait's wounds, we can affirm that, if we exclude those cases in which the extra-peritoneal treatment of the stump of fibroid tumours was adopted, his wounds healed without any complications, and his results were quite as good as the results met with in good public hospitals of the present day.

Can we explain the good results achieved by Lister and Tait? We think we can. The explanation is this: we, in recent years, have gone to an infinite amount of trouble to carry out elaborate methods for the disinfection of the skin before operations. What has been the result of these elaborate processes?

(1) That after the most prolonged and energetic washing of the skin with soap and water we find that it is not possible materially to diminish the number of microbes on the skin.

(2) That no matter how long we wash the skin it does not become sterile; and owing to the loosening of the epidermis, it really appears more infected after, than before, the washing.

(3) The use of turpentine during, or after, the washing with soap and hot water does not improve the results.

(4) An aqueous solution of carbolic acid, lysol, perchloride or bin-iodide of mercury are practically powerless to affect the micro-organisms situated on the skin; and the

use of these antiseptics, after a thoroughly preliminary washing of the skin, utterly fails to render the skin sterile.

(5) Alcohol can sterilise the skin, and far surpasses all other agents.

(6) No matter what method is employed on a roughened or chapped skin, sterilisation is impossible. If the skin is in a good cosmetic condition the result is more satisfactory, yet no matter what the condition of the skin, and no matter what the agent employed, *perfect sterility cannot be obtained.*

(7) The hands are surgically infective no matter what process is employed.[1]

Here, then, we are faced with the results that have been arrived at from bacteriological experiments; but against such results we have the word of Lister, of Tait, of Bancroft, to show that if the skin is prepared with carbolic lotion without preliminary washing, or is prepared by preliminary washing without antiseptics, that the operation can be performed with success; that is, if the patient lives, the abdominal incision may heal without suppuration.

The truth is probably contained in the following statement, founded on our own surgical experience of twenty-eight years. The skin has on its surface dirt. This dirt consists of sebaceous matter, masses of bacteria, and epithelial cells. This should be removed by soap and hot water and energetic scrubbing. If ether or spirit be used they greatly help to remove fat and sebaceous matter from the ducts of the sebaceous glands—which glands are crowded with bacteria—and when we apply a disinfectant this will more readily sink into the ducts of the sebaceous glands. But no matter what disinfectant we use, we cannot get at the bacteria in the depths of the sebaceous glands, therefore the application of disinfectants to the skin only gives us a *superficial aseptic condition which continues for but a certain limited period.* As an operation progresses the germs find their way to the surface of the skin from the hair follicles and sebaceous glands, and the skin surface becomes infected again.

Now it is to be noted that Lister always used a watery

[1] "On the Sterilisation of the Hands," C. Leedham-Green, London, 1904.

solution of carbolic during the operation, and washed his incisions with the same solution, and covered the skin surrounding his incisions with towels soaked in carbolic solution. This practice was a wise one, for Lister was a very slow, methodical, and careful operator. There was nothing rapid or brilliant about him. On the other hand, Tait was brilliant and rapid. He made a very small wound, he allowed no one to put a finger into his incisions, and his marvellous technical dexterity enabled him to finish an operation in ten or fifteen minutes, when other surgeons would have taken an hour or more.

Here, then, we think is the proper way to interpret the success of Lister, and the success of Tait.

Lister disinfected his skin with carbolic; Tait relied on soap and water. Neither the skin area of Lister or Tait would stand the bacteriological tests of the modern surgeon —say, for instance, Lockwood—but Lister covered up the skin surrounding his incision with towels soaked with carbolic, and by so doing he did not sweep into his incisions the germs that keep on working up from the sebaceous glands and hair follicles during the course of the operation. This simple precaution is one of the secrets of success in the present day.

If we have to perform an operation, when there is much manipulation—say, for instance, in the operation of the radical cure of a large inguinal hernia—we find that no matter how much care we may take with the preparation of the skin, that the wound is more likely to become septic if we do not cover the skin, right up to the edge of the incision, with gauze, either plain or soaked in some antiseptic solution.[1]

[1] This plan is always followed by Sir Berkeley Moynihan in his operations. He says, "The skin, though thoroughly prepared by Harrinton's solution, is always covered as soon as an incision is made. This I do with 'tetra' cloths held to the wound edge by the special forceps I designed for the purpose. . . . If the skin of the patient should be very rough, scaly, chapped, or cracked, its adequate preparation is almost impossible. In these conditions the 'rubber dam' introduced by Dr. J. B. Murphy, of Chicago, will be found of the greatest service. It consists of a strong, very adhesive material which is stretched and then placed in the abdominal wall, to which it clings most closely, becoming in fact, for the time, an inseparable part of this wall." "Abdominal Operations," 1914, vol. i. pp. 39, 40.

Thus Lister practised a method which we now resort to with the greatest benefit. Tait's rapidity saved him much trouble. He took such an incredibly short time to do his work that the skin about his wounds had not time to become septic from the bacteria working up from the depths of the skin.

One of the greatest of living surgeons has recently said:

> As for antiseptics, no chemical antiseptic can command good results with bad surgery; good surgery commands good results without chemical antiseptics; but the best results are the sum of good surgery and the good use of good chemical antiseptics. . . . This is why excellent surgeons may quite disregard antiseptics . . . for the same reason that Lawson Tait required no antiseptics . . . it has become the work of Sinclair and Tait to produce "rifle wounds while the rest of us produce shell-torn wounds." [1]

To sum up in regard to the preparation of the instruments, Tait's method of boiling his instruments was quite satisfactory, especially in those cases when the instruments had been contaminated by virulent pus.

As regards the preparation of sponges, probably neither the methods of Lister nor Tait would stand the ordeal of the careful bacteriological tests of the present day, and in some cases, when the sponges had been contaminated by virulent pus, they probably were not fit for use by any method then known, or in fact by any method known to us now. Hence, the great superiority of the boiled gauze sponges of the present day.

The ligatures, no doubt, were satisfactory; Tait employed only boiled silk and the peritoneal cavity is very tolerant of that substance, much more so than the subcutaneous tissues. Had he used buried sutures in sewing up the abdominal wounds he would sooner or later have had trouble.

2. We now come to the preparation of the *Instruments*, *Sponges*, and *Ligatures*.

As Lister did not boil his instruments, but relied on car-

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[1] Crile, "Surgery, Gynæcology and Obstetrics," April, 1918, p. 373.

bolic acid to disinfect them, Tait's method of boiling with washing soda was certainly superior and much nearer to the methods now universally used.

Probably the method of preparing the new sponges was sufficient with both operators. Tait certainly laid the greatest stress on the preparation of the sponges and would entrust them, as we have pointed out, to no one but his wife for many years.

Lister's catgut was probably quite sterile, and as Tait never used anything but silk, we can be sure that the boiling made it safe.

3. *Preparation of the Hands.*

Lister washed his hands in 1 to 20 carbolic lotion, or in what he called the " strong lotion " which consisted of 1 to 20 carbolic acid and 1 to 500 sublimate lotion.

This method of preparing the hands would be quite satisfactory provided the hands were not contaminated by some kinds of septic matter. We know now that, once the hands are contaminated by some septic discharges, no disinfectant can make those hands safe until some days have passed. Tait fully recognised that fact, and he was most careful not to operate on a case for some days after his hands had been contaminated. He says, in speaking about this point :

> It is a deadly thing when introduced into the peritoneal cavity by the fingers of the surgeon, even days after his contact with it. Some years ago I opened the abdomen of a woman, four days after her labour, for purulent peritonitis, and encountered this fearful putridity. Having strong suspicions of its deadly character, I soaked my hands for hours in various germicidal lotions as hot as I could bear them, till my hands were like those of a washerwoman. On the third and fourth days after, I operated on two simple ovarian tumour cases, and the rapidity with which those two women succumbed was altogether shocking; the post-mortem examination leaving no other guess than " acute septicæmia." [1]

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[1] *Med. Press and Circ.*, April 27, 1898.

One can only remark as regards Tait's refusal to use any chemical antiseptics for his hands, that he made matters harder for himself. There were periods when, after operating on a pyosalpinx, he would operate on no other case for some days, fearing lest the pus, that had contaminated him, would prevent him getting his hands sufficiently clean. Had rubber gloves been in existence in his day we think he might have worn them, because they could have been boiled along with his instruments.

To sum up, we hold that Lister's method of preparing his hands in 1 to 20 carbolic would not always prove an effective method, and that Tait's method was much worse, and we can only explain Tait's success by the fact that hands, that were clean enough for the peritoneum, would not have been clean enough for operating on the knee joint; the peritoneum absorbs rapidly, and is more resistant to mild infections than the subcutaneous tissues and synovial membranes.

(4) *Dress at the Operations.*

Neither of these surgeons dressed in a suitable fashion. Lister took off his coat and pinned a towel over his waistcoat, and Tait threw off his coat and put on a large macintosh apron that was quite impossible to keep sterile.

They both followed the same rule—they never spoke during the operation; this would give some satisfaction to the masked faddists of the present day.

(5) *The Operation.*

Lister arranged towels wrung out of 1 in 20 carbolic round the seat of the operation, and during the course of the operation washed his hands in 1 to 40 carbolic solution, and washed his wound surfaces with carbolic solution. Tait arranged dry towels, just as they came from the laundry, about, but not up to, the edge of his wounds; he used no antiseptic to wash his hands or the wound, his sponges being in plain warm water.

With regard to the above, we think that Lister's practice had some advantages and disadvantages. It was a great advantage to have the carbolic towel right up to the edge of the wound, for this helped to prevent the bacteria from

the skin contaminating the wound. On the other hand, the washing of the wound with carbolic, and the constant use of the sponges soaked in carbolic solution would at times lead to some amount of carbolic irritation, and even poisoning, and this was not uncommon in those days.

Sutures to close the Wound.

Lister used many different suture materials; Tait contented himself with silk; on this point there was nothing to object to.

Dressing of the Wound.

Lister's dressings were most elaborate, and Tait's were simple gamgee dressings.

We know from the results achieved to-day that, after inflicting a clean-cut wound in a clean surface, such as the abdominal wall, a piece of plain gauze is all that we require for efficiency; but if we have to deal with a wound from which there will be a discharge, then Lister's dressings may be more advantageous.

Thus after a close scrutiny of the methods adopted by Lister and Tait we can find very little to object to. Lister's method of preparing his hands was superior to Tait's, but Tait's extreme care about undertaking operations after his hands had been contaminated by septic matters saved him from disasters, which would be inevitable if any one imagined, for one moment, that they could get their hands sufficiently clean with soap and water after certain septic contaminations. Tait's reliance on heat for sterilising his instruments and ligatures was decidedly superior to Lister's chemical methods. There can be little doubt that Tait's wonderful rapidity helped him, and the fact that the abdominal cavity can stand much more contamination than, say, a knee joint—though this point was not recognised at that period—lent to Tait's methods a fictitious efficiency that was not founded on reality.

(D) Aseptic Surgery from Syme to Tait and von Bergmann

Having examined the methods of Lister and Tait, we are now in a position to trace the evolution of aseptic surgery.

In former chapters we have painted the conditions of hospitals in Edinburgh and Glasgow, and other large cities, to show the disastrous results that followed operations during the period when Tait was a student at Edinburgh. But it would be a mistake to suppose that the work of every surgeon was followed by these septic disasters. Such, indeed, was not the case, and it was for this reason that Tait used to maintain that many of the results obtained by Syme were equal in success with those obtained by surgeons who carried out Lister's details most carefully.

Tait has written :

> I saw some awful things in my six years of pupilage in the old Edinburgh Infirmary, things surgeons could not possibly bring about now if they tried. It is now a very parrot cry that operations are done now which never could have been done, never were attempted, before the days of Lister. This is absolute nonsense if it meant that Lister has made surgery any safer, and the evidence is at the hands of Lister's own father-in-law, Syme himself. . . . Look at his gigantic enterprises, enterprises which often, in my own recollection, brought Lister from Glasgow, and Bickersteth from Liverpool, to have the privilege of assisting. Does Lord Lister remember, as I do, the awful inguinal aneurism, in which the patient was slit, as it seemed, from the middle of the thigh up to the umbilicus, the gluteal aneurisms, the removal of the lower jaw, the fearful œsophagotomies, and, most of all, the thirty-seven consecutive cases of ligature of the femoral artery for popliteal aneurism, *not one of which died!* Perhaps most noteworthy of all, though least capable of theatrical display, were his numerous perineal sections, and all this without one single antiseptic, but with a detailed care for asepsis which was perhaps as great as my own. He would have no assistants' fingers in wounds he made ; fingers whose dirt he had no means of gauging, and no means of depriving of this deadly influence.
>
> Let us remember that Keith, who at least led us into the diminishing mortality of abdominal surgery, was Syme's patient first, then pupil, house-surgeon, and assistant.
>
> Personally, Syme was the personification of the best type of gentleman, always perfectly dressed, in his old-fashioned way, and as clean as a new pin. From his boots to the top of his head no one ever saw dirt, disorder, or the appearance of hurry. He was always washing his hands—I think I may say he washed them every time he touched a patient. His

assistants had to be like him, and his old-fashioned nurses were noted for their tidiness and cleanliness everywhere.[1]

Thus we see that although death stalked after most surgeons connected with the large hospitals at this period, yet there were exceptions, and Syme was one of the rare exceptions. He, in fact, was an aseptic surgeon by instinct, and as he would have nothing to do with abdominal surgery he was called on to perform operations where his natural cleanliness was invaluable. But there were few like Syme, and the world had to wait until Lister taught surgeons where they erred before surgery could hold up its head with any pride.

But it was a long time before men would believe in Lister and his *principle*, and during that period men like Tait arose, who, having tried to follow Lister's directions, carried out his methods more or less imperfectly, then dropped a step here and adopted a method there, and in time began to proclaim that they were *aseptic* surgeons in contrast to Lister, who was described as an *antiseptic* surgeon.

Now what did this term aseptic connote? Broadly speaking, an aseptic operation was one where chemical antiseptics were used as little as possible. That is to say that the aseptic surgeon did not disdain to wash his hands in chemical antiseptics, and did not neglect to prepare the patient's skin with chemical antiseptics; likewise his ligatures of catgut were prepared with chemicals and his sponges also, but the distinction was made that the wound was not swabbed out with antiseptic fluids, there was no spray, and the instruments were boiled and not prepared by soaking in carbolic for half an hour before the operation. Then the dressings were often plain gauze which had been sterilised in the autoclave, not the chemical-bearing gauzes of Lister.

Now who was responsible for this development?

Godlee, in his "Lord Lister," wishes to give the Germans all the credit for developing the aseptic technique; this, however, we cannot admit for a moment. The aseptic

[1] "The Evolution of the Aseptic Method in Surgery," 1898.

technique, practised by Tait from the year 1880 onwards, is practically the aseptic technique of to-day, and he certainly practised it before von Bergmann and his followers.

Godlee says that von Bergmann at first was enthusiastic about antiseptic methods, but practically became convinced that asepticism was the treatment of the future, and that antisepticism was only a transition stage. He did not, as we know, originate this idea. Lister recognised it and approached as nearly as he thought prudent to founding his practice upon it, but such a practice, if carried out in every detail, appeared to him to be neither wise nor safe. Von Bergmann, on the other hand, set his intellect to work to make asepticism universally applicable by elaborating and perfecting details, and thus became the apostle of "aseptic surgery."

We happened to be living in Berlin in 1892, and attended von Bergmann's clinic. Coming straight from Tait's Hospital, in Birmingham, we had every opportunity of comparing the methods adopted by Tait and von Bergmann. We found that the instruments were prepared in boiling water to which washing soda had been added, just as Tait had done for ten years previously; that the silk was prepared by boiling or by steaming; that the catgut was prepared by chemical antiseptics; and that the dressings were plain gauze prepared by sterilisation with heat.

Von Bergmann ridiculed the preparation of the hands by carbolic solution, and pointed out that disinfection of the surgeon's hands was not a question of the use of this or that disinfectant, but to a great extent one of scrupulous care and intelligent thoughtfulness, just as Tait had so often insisted. Alcohol was the germicide that von Bergmann liked for his hands, but he laid the greatest stress on the energetic use of soap, water, and nail brush. We found that he prepared his sponges in hot soda solution and then soaked them in sublimate solution, and that he was much in favour of using gauze sponges which could be boiled.

When the patient was brought into the theatre, the skin was washed with soap and water, and then rubbed with alcohol and sublimate. The instruments after being boiled

were placed in dishes filled with one per cent. solution of carbolic acid solution.

If during the operation the wound had to be washed, this was done with boiled saline solution.

The skin was sutured with silk sutures without any previous washing with antiseptic fluids.

Twenty thicknesses of plain gauze were laid over the whole line of sutures, and rolls of absorbent cotton placed over the gauze.

Now we maintain that Tait had brought his aseptic practice to great perfection long before von Bergmann had, and that he had reduced it to greater simplicity than the German professor had, for we see by the details given above that von Bergmann did not rely entirely on sterilisation by heat, but almost always ended by using chemical antiseptics—thus while he stated that boiling his instruments was quite sufficient, he always kept them in a solution of carbolic during the operation. He prepared his sponges in hot soda and then kept them in sublimate solution (1 in 2000); he prepared his hands with soap and hot water, but always rubbed them with alcohol.

We claim for Tait that he had brought aseptic technique to a stage that enabled him to get perfect results in his abdominal operations, and that his life's work was almost finished before 1893, when von Bergmann's complete system was given to the world in the excellent little work of Schimmelbusch,[1] the chief of his clinic.

What von Bergmann did do was to start the idea that hospitals should be built in the most elaborate and expensive manner, and that each building should stand alone. "One of the best known of these vast institutions covers 136 acres, and is like a small town. There are no corridors. Each ward is a separate pavilion with its own hypocaust, as is also the operating block, to and from which patients have to be wheeled in trollies in all weathers." From the time that von Bergmann and his followers started their ideas,

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[1] This excellent work was translated and published in London in 1894, under the title "The Aseptic Treatment of Wounds."

dates the rise of the palaces of stone and marble, and the costly operating theatres, to be seen in every big city of the world to-day—and Treves' words have often come back to us as we watched the proceedings therein: "The remarkable and extravagant preparation with which some surgeons now approach an operation, the cleansings and the washings which precede the laying on of hands, smack a little of fetish worship, and foster the cult of the Surgical Pharisee."

XXXIII

The Extension of Gall-Bladder Surgery subsequent to Tait's First Operation

We may now inquire into the extension of gall-bladder surgery after Tait performed his first operation in 1879, and we shall then be able to see what part Tait took in this extension.

When Sims performed his first operation on the gall-bladder, he introduced the term *Cholecystotomy*. As the various operations on the gall-bladder and ducts were performed, there sprang up a number of terms all of which were created on Sim's original plan. The first of these terms sprang into being when Langenbuch, after performing many experiments on the cadaver, determined to remove the gall-bladder from one of his patients, and so we get the term "cholecystectomy." [1]

Let us admit at once that to remove the gall-bladder, when that organ required to be removed for disease, was an

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[1] Moynihan, in referring to the operation, makes the remark: "The year 1882 was the most memorable of all in the development of gall-bladder surgery, for it was in this year that Langenbuch first performed the operation of cholecystectomy. To Langenbuch, as much as to any surgeon, belongs the credit of establishing the surgery of the gall-bladder upon a firm footing."

Now why Moynihan should want to wander away from England to find a German on whom to bestow the honour of placing gall-bladder surgery on a firm footing we cannot guess, unless it be that he exhibits on every occasion a jealousy which blinds him to the merit of Tait's work. Tait taught Mayo Robson how to operate on the gall-bladder, and Mayo Robson taught Moynihan, who should acknowledge his indebtedness to Tait, but he never mentions his work if he can possibly avoid it. Thus, in the historical sketch in the chapter on operations upon the gall-bladder and bile ducts in his book on "Gall-Stones," Moynihan alludes to Tait's pioneer work in the following words: "In England, the first surgeon to operate deliberately and with success for gall-stones disease was Lawson Tait." He gives him no credit for being the real founder of the surgery of the gall-bladder, but, as we have mentioned above, wishes to give the honour to Langenbuch to whom "belongs the credit of establishing the surgery of the gall-bladder upon a firm footing."

advance in gall-bladder surgery, but at the time when the operation was first introduced the operation was often employed when the simpler procedure of cholecystotomy would have done quite well, because, as Moynihan remarks: "the experience of every surgeon, who has worked extensively in the field of surgery, is that the chief purpose and the main indication in any operation for gall-stones is the drainage of the gall-bladder and bile-ducts;" and this was the point that Tait insisted on.[1]

Mayo Robson says, "that many surgical advances, especially in recent years, have been carried out with more zeal than discretion, and in several well-known instances the bounds of good judgment have at first been far passed, though ultimately moderation has prevailed."[2] These remarks were written in connection with cholecystectomy, and Mayo Robson further says that he protests against the indiscriminate removal of the gall-bladder in all cases in which an operation on the bile passages is called for.

Since 1892, when the first gall-bladder was removed, until the present day there has been a very considerable difference in opinion on the point whether the gall-bladder should be drained or removed. Tait did not take kindly to the idea of removing the organ, and in 1885 wrote to the *British Medical Journal*[3] the following letter:

CHOLECYSTOTOMY *v.* CHOLECYSTECTOMY

SIR,—I have just had my attention drawn by my friend, Professor Boddaert van Custen, to a recent discussion upon this subject at the Belgian Académie de Médecine, where it was argued against the operation of cholecystotomy, and in favour of that for removing the gall-bladder, on the ground that the recurrence of stones in the gall-bladder would be likely, by leaving the organ.

This is either a merely *à priori* objection, or it is based upon actual experience. If it is a mere *à priori* statement, it may be dismissed at once without discussion, because I desire to

[1] William Mayo, however, says: "Experience teaches that unless there is obstruction with jaundice and definite cholangitis the value of bile-drainage to the surface, following operations on the biliary apparatus, has been overrated." *Surg. Gyn. and Obstet.*, February, 1921, p. 99.

[2] *Brit. Med. Journ.*, June 13, 1885, p. 1224.

[3] *Brit. Med. Journ.*, June 13, 1885, p. 1224.

place it on record that not in any one of my cases is there, so far, the slightest evidence of a tendency to recurrence of the disease. The earliest of these cases dates from August, 1878. Fifteen of my cases are still alive, and I do not know, with the exception of my colleague, Dr. Savage, that there has been any other successful trial of the operation. I desire also to point out that, even if the gall-stones were to form again, their subsequent removal, by a small incision through the old cicatrix, would be the easiest possible proceeding, as, in my operation, very careful union is made between the gall-bladder and the abdominal walls. The operation for the removal of the gall-bladder seems to me a radically absurd one, and wholly unnecessary. In the first place, it is terribly fatal, because, so far as we can learn the statistics of it, in the hands of its originator at least fifty per cent. of the cases have died, whilst all my fifteen cases of cholecystotomy have recovered, and with one exception have been cured. The formation of gall-stones is not due to the existence of the gall-bladder, because we find gall-stones in the substance of the liver and in the biliary duct. It is due to some morbid condition of the secretion of the liver by which there seems to be maintained in the bile a superabundance of cholesterin, or of some substance or substances which deposit crystals of cholesterin. Removal of the gall-bladder would not in any way contribute to an alteration of this condition, and the mere existence of the gall-bladder is satisfactory evidence that it has some important function to fulfil, though what that function is yet remains to be discovered. Removal of the gall-bladder, in the abnormal condition of the bile which induces the formation of gall-stones, might have a really deleterious influence, for all that we can tell.

The opinion expressed by Tait had the support of Mayo Robson in 1909, while William Mayo, in 1915, wrote :

> In comparing cholecystotomy with cholecystectomy it should be noted that cholecystectomy has a slighter higher mortality, and that the increased death-rate does not depend upon causes which can be eliminated but rather which are inherent to the operation itself. In our experience the ultimate result for the patient following cholecystectomy have been no better than in cholecystotomy under similar conditions. [1]

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[1] Keen, " Newest Surgery," 1913, vol. vi. p. 1603. Recently (February, 1921), William Mayo said : " Cholecystectomy has taken the place, to a large extent, of cholecystotomy because of the well-known fact that the gall-bladder, following cholecystotomy, is a liability rather than an asset to the patient."

It would appear that at the present time there is a decided tendency to remove the gall-bladder, where before it would merely have been drained. This matter cannot yet be finally decided; there are many cases where drainage is quite sufficient, while there are certain cases where removal of the gall-bladder is imperative; the future will unfold to us which cases should be drained, and which cases should have the more radical operation.

Cholecystendesis. The second modification of Tait's cholecystotomy was suggested by Sir Spencer Wells, and he proposed to open the gall-bladder, remove the calculi, and then close the bladder by continuous sutures without attaching it to the abdominal wall. Tait, writing of the proposal in 1889, says:

> So far as I know, this proceeding has been but seldom put in practice, and has been fatal in every instance. In one case the post-mortem records of it have been made public. A large quantity of bile was found in the peritoneum, as might have been expected, and whether this was the cause of death or not, it certainly was not a condition which we might expect would be contributory to success. But the arguments against this proceeding are *à priori*, and are of the strongest possible nature, and do not depend merely upon the want of success in a few preliminary cases; in fact, the arguments against it are so strong that it is difficult to see how it ever came to be suggested.[1]

This so-called "ideal" cholecystotomy was first performed by Wells' colleague, Meredith, with fatal results. This operation is now never performed.

Choledochotomy. When a gall-stone has passed out of the gall-bladder and is lodged in the common duct it is removed by performing choledoctotomy. This was first suggested by Langenbuch in 1884, while Kummell about the same period, after performing cholecystectomy, said that he had removed a stone the size of a walnut from the common duct: his patient died. The first successful operation was performed by Knowsley Thornton, in 1889, while Courvoisier performed the operation with success in three instances in 1890.

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[1] *Edin. Med. Journ.*, October, 1889, p. 310.

Tait does not appear to have performed this operation up to 1889, for in a paper on the "Surgery of the Liver," [1] he says, in speaking of biliary fistula, "that such a fistula after cholecystotomy must be due to the fact that the operation had been performed at a time when a gall-stone or gall-stones had become impacted in the common duct. In three or four cases I have crushed this obstructing gall-stone and have succeeded thereby in getting a free road through the common duct." He calls this operation cholelithotrity, which he says means the crushing of a stone outside the walls of the duct by means of padded forceps: to-day the operation is termed choledocholithotripsy. Tait performed this operation for the first time in 1884.[2] It is never used now except when the stone is in the hepatic duct—hepaticolithotripsy.

With regard to numerous other operative procedures on the gall-bladder and ducts, we need but mention them briefly. Tait does not appear to have practised any of them.

In 1882 cholecystenterostomy was first performed by von Winwarten, after the procedure had been suggested by Nussbaum. In this operation the gall-bladder was united to the colon: later on Bardenheuer and Terrier carried out the suggestion of Cozi that the gall-bladder should be united to the duodenum. Later on the bile duct was united to the duodenum by MacBurney, and this gave rise to the term "duodenocholedochotomy."

In a few cases a portion of the common duct has been excised, and the ends joined together. Doyen, for instance, excised the damaged portion of the duct, and joined the two ends of the duct completely without drainage; and we ourselves have anastomised the hepatic duct to the common duct.

The surgery of the gall-bladder has to-day reached very great perfection, and no longer has to be defended from the attacks that were made against abdominal surgery in Tait's time. In those days (1885), he had to write the following passage in a paper on the "Surgical Treatment of Gall-Stones":

[1] *Edin. Med. Journ.*, October, 1889, p. 312.
[2] *Brit. Med. Journ.*, July, 1884, p. 67.

We who practise abdominal surgery are told that we have no business to perform these operations, save in cases where life is absolutely threatened; . . . our functions are not merely of the life-saver, our greater and much more successful function is the relief of pain and suffering. Among all the many advances which modern abdominal surgery has seen, I claim that there is none so certain, nor so free from risk, nor so brilliantly successful, as the surgical treatment of gall-stones.[1]

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[1] The *Lancet*, September 5, 1885.

XXXIV

Abridged Report of the Address in Surgery

By Lawson Tait, F.R.C.S., *President of Mason College, Birmingham*

1890

Mr. President and Gentlemen, I do not know that there is any position in which one who practises the handicraft of surgery could feel a more weighty responsibility than that which I now occupy, and if responsibility is the best measure of dignity, I frankly acknowledge that there is no position in the whole wide world that could by any possibility be conferred on me, which I should regard as a higher honour than that which I am about to try to fill worthily in addressing this great Association on the subject and object of my daily life.

That we have made mistakes, that we have had to confess that our favourite theories and some of our best established practices in one generation have become the flogging posts of the next, is but to confess that we are human. Nothing is human if it can give no progress, and progress is impossible in all human affairs without error. Even the most perfect of human sciences —mathematics—has had, is having, and probably will have in time to come, its evolutionary stages. Even the apparently finite doctrine accepted unquestioningly for nearly two thousand years, that two lines which are parallel can never meet in space, is being shaken to its foundation, and those who can approach space from the aspect of its fourth dimension, are staggered to find that the shortest road between any two fixed points may not necessarily be in a straight line. With such awful examples before us ; with the fear that even the treasured first book of Euclid's propositions may prove to be a fraud, we who practise a mere handicraft, but one of the utmost importance to humanity, have need to walk carefully.

It is not necessary that I speak of the importance and dignity of our work, for that is admitted on all hands, even with full knowledge of our shortcomings. Admitted it is to the full, for there is none unwise enough to avoid the counsel and help we can give him when the inevitable hour of trouble comes for his turn. The scoffer may be what he likes when he is well, but when the abscess pains of the tumour threaten, his tastes speedily become monastic. Humanity has on the whole a complete confidence

in us; not that we are perfect, but as far as we are so approached we honestly do the best we can. It behoves us, however, from time to time to search out the innermost corners of our chambers, to rid them of all uncleanliness, and then to garnish them and to set them in order, hiding no blemish, concealing from ourselves even no defect, but striving in all ways to the perfection of that noble work to which we have set our hands.

You have already been welcomed to this great human hive in the words of our eloquent President, and I wish to re-echo his welcome because I have for once in my lite an audience before me worth preaching to, and I wish that my sermonising powers were equal to the occasion. You have come to a great home of industry, the very paradise of the handicrafts. I wish I had the tongue of a William Morris to sing their praises, but as you go through our workshops, even those of the humblest kind, you will see such wonders as would empty from sheer exhaustion even his silver store. To look at the work, you may think that as little skill as exists in human sense would be required to cut an ivory nut into what they call blanks in a manufactory of buttons; but as you watch the patient workman for a few hours, and then think you would like to try to rival his skill, you take your place at the saw bench, and if you are not preternaturally skilful it will be only after sacrificing a few of your fingers that you find yourself behind him inexplicably in every conceivable way; you would then begin to have some slight appreciation of the skill he has acquired even in that small department of human art. You would then begin to appreciate the principle we all value here so much, for it constitutes the making of our factories—I mean the principle of the subdivision of labour. In such an apparently rough, and certainly very humble, employment as that of the cutter of the button blanks which I have taken as my text, a few questions will help us to much thought, and the answers may excite in many of us solutions for problems which are greatly exercising the authorities of our medical corporations at the present moment.

You will find that even the cutter of the ivory nut will prove to be a more docile apprentice and a more competent workman if he has had a good education before he enters the shop, such an education as we supply now in our Birmingham schools, in a fashion compulsory and almost free. I wish it were entirely free. The philosopher who asserts that liberal education will spoil the future craftsman is a mere bookworm, without a true knowledge of human powers. We further find that our button cutter must go through a long apprenticeship to accustom his eye to judge the nut, to avoid its faults, to accustom his fingers to the sense of resistance, and even his ear to the sound of the saw. Mere general education will do nothing of this kind for him, but the general training he has had will open his mind first

of all to the sublime advantage of accuracy in all things, and it will make his mind just so much the more receptive for the monotonous details of his work. Finally, it will make him in all respects a better citizen than he would have been without it.

A surgical craftsman must be a trained gentleman, accustomed by a classical education to use his native tongue with ease and fluency, and without confusion. He must have the fundamental principles of reasoning and of business habits instilled into him by such mathematical training as will be involved in his being able to pass some one of the ordinary examinations now insisted upon by all the licensing bodies. If he can spare the time and money to become a graduate in arts so much the better. Up to this point we are all agreed. Our apprentice surgeon has now to enter upon his purely professional training, and to learn the constituent parts of the body and their functions ; and here comes in our first difficulty. I, for one, desire to raise my voice in protest against the absurd attention to detail and the enormous waste of time involved in the present biological training of the surgeon student. Let him be grounded in every fact of anatomy which may, under the rarest and most unlikely conditions, aid him to appreciate the results of an injury, or a displacement, or of a new growth ; let him be grounded in all such items of information concerning the ultimate structure of organs and their mediate and immediate functions and the changes to which disease subjects them.

Let him be placed so constantly alongside somatic sections that he will not only learn his anatomy, but that he will never forget it. Let him see things and think of them so often that he will, as it were, see through his patient as this man sees through his nut before he cuts it up. But I plead most earnestly that your successors shall be spared that senseless grind at useless details of anatomy with which our own young memories were burdened—details which he can remember only by a demoralising system of catch words—details which he prepares himself to forget the moment the necessity of examinations is over.

Still more strenuously I appeal that our student be altogether relieved from that senseless system of biological training which has set in as a fashion at Cambridge, at Oxford, and at Edinburgh. Not many years ago I attended a lecture on physiology given for medical students, which consisted in an explanation of a brass instrument resembling a model of Clapham Junction, intended to explain something about muscular fibre. I could not understand it, of course, I was too much of an old fogey, but I had this consolation, that when talking it over with my young friends who had attended the lecture with me, they could make nothing of it either, and it worried them as much as it had worried me. But there was a difference between us, it was demoralising to them, for it discouraged them, and small wonder.

And how angry they must feel when they come to deal with human patients and human disease, that all these nonsensical details are of no use to them—not even for the purpose of general training; when they find, in truth, that the time occupied in mastering such subjects has been absolutely thrown away. For students who are disposed to appear for a science tripos, or who have such a line of life open for them or the tendency towards it, who are possible professors of anatomy; medical students, nine hundred and ninety-nine out of every thousand, will have to find their positions at the bedsides of their fellow-countrymen in times of accident and sickness, and then such knowledge is useless.

I remember that we had to learn that the direction of the anterior cornu of the fourth ventricle of the brain ran a course which was backwards, outwards, downwards, forwards, and inwards, and we were enabled in the most improper way to remember these important facts by the word "bodfi." Has "bodfi" ever served any of you at the bedside? Is there any conceivable condition of human accident or ailment in which "bodfi" could assist you to relieve your patient? The students who continue to learn such matters will find, as I have done, that they will be of no assistance to them to estimate the character of a delirium, and no amount of knowledge of the arrangements of the electrical currents in muscular fibre will help them to determine the proper relations of a splint. What I wish for our students is that they should go back to the institutes of medicine and leave comparative biology to those who may be able to benefit by it.

In the old days—days which I can remember—it was charged against the corporation that they turned out a large number of ill-educated empirical practitioners who knew nothing but their patients. Now I say the tendency is to turn out a still larger number, a very much larger number, of scientific young tyros who know neither patients nor their diseases till they have gone through a second pupilage extending for years after they have left their university. This second pupilage lies in the rough school of experience, and in its second training they will be forced deliberately and at once to throw overboard at least two-thirds of what they have learned in the first. What the boy wants after his general education has been fully developed, and his fundamental knowledge of useful anatomical facts and physiological principles has been made perfect to the utmost of their extent for usefulness and not one scrap beyond that, is that he should be dealt with as we deal with the cutter of blanks in the button manufactory. He should be put at once into contact with his material. I therefore vote cordially with those who demand the restoration of the apprenticeship system in such fashion as modern requirements indicate. It is, of course, no longer to be a seven years' slavery in mixing pills and spreading plasters, for the modern manufacturing chemist does all that for us now,

but it should be a period of at least two years spent in learning how to deal with patients, how to divine their peculiarities, and in learning how to avoid making an ass of himself in the sick room as the modern, newly fledged, qualified assistant is certain to do for the first few years of his second pupilage in spite of his biological lore.

Upon one point we are coming to an agreement, and that is a demand for the restoration of the practice of the apprenticeship. But I would carry it further. While the student is attending to this most important part of his training, he ought at the same time—and now I am speaking for those who have to follow the craft of surgery—to be taught how to use his hands. I should set him so many hours in the week into the shop of the village carpenter ; and I would have him trained to use a saw, a chisel, a plane, and a skew so that he should be able to make a long splint if need be, as well as to put it on. And into the blacksmith's shop he should go, till he knew how to strike properly with a hammer. Some of you may think this may be unnecessary ; but if you could look with a workman's eye (as I can do, for I served my time at the lathe, the bench, and the forge) at a Fellow of the College of Surgeons—I won't give his name, but you will find him in almost every large hospital in the kingdom—who used a saw for the first time in his life in the amputation of a human leg, and see, as I can see, what a horrible mess he makes of his work, you will agree with me that a training in practical mechanics is just as necessary (I say it is far more necessary) for a man who has to operate upon his fellow human beings as is a training in anatomy.

Having now delivered my soul concerning the training which I believe to be essential for the craft of surgery, I fall naturally into the utterance of some words concerning the practice of the art, and I pause at the outset in absolute wonderment when I contemplate what has happened in my own lifetime. The year 1847 was the real birth-date of our work, when there first fell upon man that gentle slumber, anticipated in the beautiful legend of the Garden of Eden when Adam's rib was resected, but which was not brought into the region of actual fact till that memorable night in Queen Street, when Simpson, Keith, and Mathews Duncan " were all under the table in a minute or two," to quote Simpson's own graphic description. What had gone before that time was mere groping in the dark, and what has happened since could never have been ventured upon but for that greatest of all human blessings—anæsthesia.

That has been to surgery what the motive power of steam has been for the arts, manufactures, and for commerce : it has revolutionised everything in connection with our art. And yet we are so accustomed now to take the advantage as a matter of course that we have almost forgotten its history. We are apt to ignore

the fact that all our brilliant advancements of to-day could never have been arrived at but for chloroform—we could not have developed the splendid work of the modern ophthalmic surgeon, and the modern development of abdominal surgery would never have been dreamed of but for the genius and indomitable fighting qualities of James Young Simpson, who thrashed out the victory of anæsthesia and gave us the anæsthesia which for more than half a century has held its own against all comers.

This, the greatest of all medical triumphs, at once broke down the barriers which had hindered the development of our art, and a vast change in surgical practice became apparent. Not only were the horrors of the surgical theatre banished, but the type of men who could and did practise surgery was altered—in some instances though, not for the better. In the days before chloroform the man who "niggled" over an operation was avoided alike by patient and doctor. A man like Liston, whose manual dexterity is said almost to have approached the miraculous, had it all his own way. Syme returned from London to Edinburgh for many reasons, but no doubt chiefly because his comparatively defective manual dexterity was very apparent in contrast to many of those with whom he had to be in rivalry. He never could cut decently for stone to the end of his days, and but for the aid of the chloroform of his greatly disliked colleague, we never could have had the splendid philosophical principles of surgery which Syme laid down. He never could have been the surgeon he was without the encouraging influence of an anæsthetic.

In fact, the history of this epoch of surgery is one of the most interesting there is, and I venture to say by far the most important. Indeed, it is only by reason that we are more or less all part of it that we do not see how overwhelmingly important the transition effected in the years between 1850 and 1865 has been in the development of our art. In that time appeared men like Paget and Savory who never could have been surgeons as Liston, Fergusson, and Miller were, but who lived, and mostly are living still, to fill a higher and more useful post. They became the surgical pathologists, among the first of whom in date we have to put Stanley, and now they are a goodly army. They have lifted us from mere handicraftship to something approaching a science—approaching the position of a science more and more every day.

But in addition to this, probably the best result of the change effected by anæsthetics, another of great interest arose about the same time. With the possibility of human benefit came a huge extension of the number of cases, not only of those suitable for the new work, but of those who could bring themselves to submit to it. Before the days of chloroform vast numbers of men died of stone in the bladder rather than submit to the torture of the operation. Nowadays nobody is foolish enough to refuse to

submit to the operation. Operations were comparatively rare and they were made the subject of vulgar theatrical display. I remember that when Syme had to tie an iliac artery or remove a tongue he would have seven or eight hundred people from all over the country to "assist," as the French say, for only two or three could see what he was doing.

This strange perversion survives even to this day, and it makes me often sad to hear of the anxiety professed by many of my profession to "see" some great surgeon perform some big operation—an operation which they themselves would never entertain a wish to undertake. Such performances, I think, ought to take place only in the utmost privacy and be witnessed only by such as can really be benefited by observing them, that is, men who wish to engage in operating practice and are likely to have fields in which to fulfil their wishes. Of course there are some operations to be excepted from such remarks, and the real intention of what I have said must be clear enough to every one —for an operating surgeon to have a theatre full of second-year students merely for the purpose of being able to say that they were in the same room with him is a useless waste of time for the students, and for such purposes I wish all operating theatres were closed.

In abdominal surgery nothing was really done, if we except the truly brilliant achievement of Ephraim McDowell, before the days of anæsthetics. It is true that in this country a few par-ovarian cysts were removed and a still smaller number of very simple operations for ovarian tumours were completed; but there is little doubt that whenever any serious complication was met with, the abdomen was promptly closed. No solid tumour was completely and successfully removed till that now before us fell to the hands of my valued old friend, John Day of Walsall, who was gathered to the majority only a few months ago. The greatest advance of all—the intra-peritoneal treatment of the pedicle by means of the cautery, in the hands of Baker Brown giving a mortality of ten per cent.—was the real starting-point of all our progress, and that proceeding would have been an absolute impossibility without the aid of an anæsthetic. Then came Spencer Wells, who, with his precise methodical plane and steadfast business-like habits which have characterised all he has done, has established abdominal surgery as a distinct speciality. For this as well as for much more, I and others owe him much gratitude.

As the practice of surgery has grown, so have the arrangements and provisions for its pursuit become enlarged and altered to suit the varying requirements. Great changes have been necessitated in our hospitals and in our medical schools, changes again to be traced directly to the inestimable benefits of anæsthetics. When I came to Birmingham in 1870 there was practically no

hospital between Summer Lane and Stafford, or between Bath Row and Worcester. The towns round about us were supplied by old-fashioned dispensaries, where bronchitis was treated in the winter and diarrhœa in the summer. Serious accidents and important surgical cases were all jolted into the General and Queen's Hospitals. Old John Burton, of Walsall, aided by Sister Dora, had just begun to show, in 1870, what a cottage hospital could do. Now we have not only cottage hospitals innumerable, but we have palatial receiving houses for accidents and urgent cases at Dudley, West Bromwich, Walsall, and Rugby, and many other important centres. These local hospitals would do credit to any community, and the surgical practice in these institutions is quite brilliant, as many of you know. We have now in our midst a hospital for diseases of the eye, superior to anything of the kind, and its four surgeons engaged exclusively in special work, are not excelled in the excellence of their results throughout the wide world. Not only so, but ophthalmic hospitals have appeared in smaller centres, as Shrewsbury and Wolverhampton, which have already acquired highly deserved reputations under the care of men who do not engage in special practice nor in general surgical work. A new feature has appeared at West Bromwich, for which I predict a rapid extension and splendid results. An enlightened committee has engaged the services of a special ophthalmic surgeon from a distance, at quite a handsome salary, to attend to their ophthalmic cases. The same committee has for many years engaged my own services for special cases, with the rank of consulting surgeon, but as the cases are few the handsome salary in my own instance is yet merely a prospect. In this same line another well-pronounced division of surgical work has been accepted in Birmingham and the Midland district, of which you have heard something in another place from the lips of my highly esteemed colleague, Dr. Savage. Here, more than anywhere, can be seen the triumph of anæsthetics, for not one of our modern advances could have been accomplished without them. Jean Louis Petit planned and described in detail the method of curing the troubles of gall-stones, 1745, but they could not be carried out till our patients were soundly under chloroform.

The rearrangements of hospitals necessitated by the changes just described have had a most wholesome result on the practice of surgery. The crowding of our two large hospitals by cases of compound fracture and amputations has been sensibly diminished, and the cases are treated far more safely, and quite as well, in the cottage hospitals nearer the place of accident. The surgeons to our large hospitals are becoming much more of the special consulting and operating surgeons, and less of the senior general practitioners than they used to be; and in our smaller centres are being trained men, competent in the highest degree, to deal with the ordinary surgery of everyday life. In their

turn these men are recognising the advantage to their patients and themselves of yielding up the grave responsibility of special and exceptional cases.

By the development of specialism, and the establishment of a set of men in our midst who are not in any way engaged in general practice, I venture to think a great many advantages have been gained. Of this, not the least is that the public derive what benefit consultation may give at the onset of their ailments instead of at the end, for I venture to say that nowhere now in this district are serious abdominal symptoms encountered without a speedy summons being sent to one of us who practise in that department; and it is in no small degree owing to the foresight and prompt action of our brethren who seek our help that we have the success which has crowned our efforts. Unless those who practise such a speciality have the confidence of their brethren by keeping clear of general practice, no such satisfactory results can be gained.

In dealing with the third division of my subject I have less clear ground upon which to go, and I fear I must depend more upon what I wish were the facts than upon what can be actually shown in their present existence. But I sincerely wish that we had better surgical results than we have; I wish we knew more exactly what they are; and, most of all, I wish that we could go about establishing them in a more logical fashion.

It has often been said that the greatest of the gifts of God which take the form of genius, I mean oratory, is the most potent of all for immediate effect, but most transitory for fame, and so it is with the sister histrionic art. The dead printed matter (all that we have left of John Bright) gives not the slightest idea of the great artist he was. The phonograph may alter all this, and the orators of the future may enthral audiences as much as the skill of Leonardo captivates you in the Refectory delle Grazie at Milan, just as it could have done four hundred years ago. We have no such promise for the fame of the surgical artist, for he and his handiwork perish alike, leaving nothing but the bald and printed statement of cases. The limner's work lives almost for all time, and the sorrows of Ursula affect us quite as much now in the hospital at Bruges as when Hans Memling first depicted them. We have only one comfort. In the days which Edward Bellamy predicts, and for which William Morris pines, the limner and the surgical artist will be the only highly paid craftsmen, for their art will remain something unapproachable by mechanical contrivance. This may be or may not, but it is a compliment of no small excellence that so great an authority ranks our craft so high.

In the results of our work we have much cause for congratulation, but I would rather look on the other side of the fence and wonder if they could not be improved, even with the methods

we have now at work. For the last twenty years we have first been opposing a great theory, then we have been accepting facts on which it was based, then we have been rushing into violent and illogical enthusiasm about it, only at the end of all to throw doubt and dispute on the whole field. I confess I always doubt surgical theories, just as I doubt all theories of art. The greatest painters have been the least trenchant about theories. Turner never had a theory of any kind, and always used bad pigments; and John Brown tells us that Guido mixed his colours with brains, and could get no further explanation of his results. No school of art which started on a theory has ever made a lasting impression. The modern school of the pre-Raphaelites gathered adherents, it is true, but they have nearly all deserted their theories on finding that the Dutch school knew far more than they did; the Flemings had all their excellencies without falling into their errors. The impressionists were similarly a failure. Our surgical theories never lead to anything; not even the great antiseptic theory has led to any tangible result, beyond what every housewife knew before its day, namely, that dead moist organic matter will decompose if some agent or other gets to it. We know now the exact nature of this agent, but this is a new fact, not a new theory. The theorists forget that living tissue will not decompose under the access of the same influences—influences, indeed, which surround us at every moment of life, and pass by harmlessly. Now the theorists take a lingering farewell of their lost darling by saying: "Well, at any rate, it taught us cleanliness." As a matter of fact, the very reverse of this is true, for it was the arguments of those who opposed the antiseptic theory which demonstrated the successful cleanliness. The last phase of this discussion—I sincerely hope the very last—is the antiseptic accoucheur, who pleasingly fancies that both his theory and his practice are new, whereas, in matter of fact, Semmelwies literally died for them nearly thirty years ago. No more instructive reading can be indulged in than a brief monograph which has recently been issued concerning the history of this truly great man—a man so great, that I think he deserves to have erected to his memory a statue in every civilised country. Semmelwies had no theory, he simply stated the fact that puerperal women in Vienna were poisoned by dirt. "Wash your hands," he cried, "and the women will not die," and his colleagues ruined him for his frankness. But he persuaded the world he was right. Simpson took up the fight with his accustomed vigour, and carried it through, and now, forsooth, we hear of the antiseptic theory as applied to midwifery as a new thing.

What is wanted for the improvement of our surgical results is not more theories, but better work and better systems of working, preceded by better systems of training. We forget that an art like surgery cannot be acquired by passing examinations.

It is true that examinations admit to our academy schools in art, but they are merely for the purpose of selecting candidates upon whom it is likely that benefit will arise from the teaching which there can be obtained. Surgical art is not conferred upon the successful candidates by the College of Surgeons' parchment for the Membership, still less by that for the Fellowship. Like all arts, surgery requires some indescribable gift, easily recognised and appreciated when seen, but wholly incommunicable by diploma or otherwise. With that gift the art may be acquired by persistent labour, but without the gift the labour is hopeless, and that this fact is being recognised in all our large centres of population, even in the close hospital corporations of the metropolis, is one of the most hopeful signs for better surgical results. Another hopeful sign is the demand for complete and accurate statements of results. Public attention is now being drawn very largely to the question of hospitals in general, and a most wholesome rivalry is being established upon the question of results ; so much is this the case that grave suspicions are being entertained regarding all institutions which neglect or refuse to publish their results. I remember very well, that when engaged some fifteen years ago in writing a book on hospital mortality, I asked a large metropolitan hospital to furnish me with the facts of their amputations for a period of sixteen years. The hospital was notoriously reticent upon all such questions. The medical board answered my request by a resolution that it was not expedient to furnish me with the information required. I replied that if they did not furnish me with what I wanted within a month, I should publish their resolution in the *Times*, and, as this hospital was entirely dependent on annual subscriptions, they reconsidered their decision. Their amputation results were ghastly ; probably they did not themselves know how bad they were. Publication of these results had a marvellous effect, for now the same hospital has an amputation mortality of less than one half what it was sixteen years ago.

To Sir Spencer Wells is most undoubtedly due the credit of putting the publication of surgical results on a business-like basis, and the extension of this ought to be encouraged in every possible way. When the results in abdominal surgery, which began to be obtained about twelve years ago, were first published, they were so amazing that the favourite criticism of them was that they must be lies—a complimentary method of criticism which, it may be mentioned, was meted out to Ephraim McDowell. This sort of thing was very irritating and used to make me very angry ; but for many years past I have ceased to trouble about it, and the incredible things of eight or ten years ago are now matters of everyday experience. My present desire is to urge in every possible direction a careful classification and publication of surgical results as one of the best methods of improving them,

for it is clearly necessary, first of all, that we should know what our results really are before we can see the necessity for their improvement; and, secondly, it is only by the comparison of the results from different hospitals and different surgeons that it is likely that we shall start rivalry and inquiry as to the causes of the better results.

Another great advance required is the devising of logical plans in recording and classifying the results, and also the adoption of reasonable methods of conducting the investigation. For example, no one would now, in making a research on the mortality of amputations, arrange his figures without a careful separation of amputations for accident and those for disease, and a perfect arrangement of the cases according to the limb affected and the point of amputation. But until Simpson pointed out the necessity for such divisions, the importance of them was not clearly understood, certainly was not fully admitted. Arising out of a matter so simple as this there are, there must be, scores of points which require settlement, yet no serious attempt has been made even to indicate what such points may be. It may be, for instance, that one particular method of flap making will suit primary amputations better than another, and a point so important could be settled in twelve months by a simple agreement amongst a group of hospitals and their surgeons. Let one set of men work steadily at one method for one class of cases, and another set at another method, and the statistical laws will be found, faithful to the truth, as they ever are when the figures are large enough. Instead of adopting a method like this, our present method is, that when such a point is raised for discussion, surgeon after surgeon rises and gives vent to vague impressions which he is pleased to elevate by the name of opinions. The fact is, that every one has been trying all the plans in a disconnected illogical way from which truth never can be evolved. One of the many things I admired about Tom Keith was the religious way he stuck to the cautery in treating the ovarian pedicle. Nothing could shake him? I was equally obstinate in my adhesion to the silk ligature. The result was, that when we came to compare notes after many hundreds of ovariotomies, we found that so long as the bleeding from the pedicle is effectually stopped, and the pedicle carefully dropped back into the peritoneal cavity, it does not matter a pin how it has been treated; and that I venture to say is a surgical conclusion, not only of the utmost importance, but one which never can be contravened.

The so-called discussions on special subjects which are now fashionable at many annual gatherings illustrate well how utterly futile our present method of research is. Take the case of the kidney. We have half a dozen men discussing such a question as that of removing a suppurating kidney as a primary operation or subjecting it to a preliminary drainage, and when you have

heard all that they have to say you are no wiser on the subject. But if four men would subject all their cases to one method, within five or six years the question would be definitely settled, and the probability is that we should have determined as a definite conclusion that in all certain well-defined cases the best treatment was the first method whilst in another class we should probably discover that the alternative was the better choice. The latest instance of this kind is the question of lumbar versus inguinal colotomy, concerning which I am certain that the shield has a silver as well as a golden side, and that our present method will never enable us to differentiate the two metals.

Some such plan of research as I have described would very clearly indicate in which set of conditions which proceeding ought to be adopted. It may be urged against my proposal that such a plan or research would hamper liberty of action, but I answer that our present liberty of action is not wise ; indeed, it is not liberty at all, but licence.

Let me take a personal case, it is well known that I adopt a special method of treating uterine myoma, and that there is a rival in the field in the shape of the electrolytic method—a charge to which I readily plead guilty—and for this conduct my defence is simple. I say that no logical and complete conclusion can be arrived at by everybody treating everything in every kind of way. Having opened out a continent I want to know all about it. I pursue, therefore, all the windings of its rivers, and I measure the heights of all its mountains, and I give you the results of my ten years' wanderings. Let the electrolysians do the same, and then you shall be the judges, not upon men but upon principles. If the primary results of the removal of the uterine appendages are satisfactory enough to justify the experiences being continued over an area of about four hundred cases the judgment will rest upon the ultimate and permanent results. If the results of electrolysis are given with the same fulness and with equal authentification, and are found to be better than those of surgery, I have no more to say on the subject, and shall take a back seat. But meanwhile, I am sure, in the interests of our art, that it is better that I should continue my research in a logical fashion, unhampered by qualifications which would make a just conclusion on your part an absolute impossibility, and which would lead to nothing in my own mind but confusion.

Similarly when removal of the uterine appendages was proposed, but long before the just and true principles on which it is now based could be formulated, everybody rushed into the trial of it and the result was a disastrous epidemic, the chief burden and discredit of which fell upon me.

I was more horrified than I can tell, and much of my time was taken up in disclaiming the doubtful honour of what was called

"Tait's operation," in the performance of which every principle advocated by Tait was neglected or deliberately outraged. If the men who engaged in this work had waited for a reasonable trial, a fair discussion, and a just verdict, much discredit for our art would have been spared us. The just verdict has now been arrived at, and the misrepresentation of which this operation was the centre has now ended. But the example is a very telling one in illustrating the want of logical application in the present method of research upon surgical results.

XXXV

Work done between 1891 and 1899

We have now arrived at the year 1891. The event in the preceding chapter—the delivery of the address on Surgery at the fifty-eighth annual meeting of the British Medical Association—may be set down as the high-water mark of Tait's career. Although he continued to go to London, and attend the meetings of the various societies to which he belonged, he communicated but few important papers during the remaining years of his life. After 1890 his original work may be said to have come to an end. He made no new discoveries; he dropped out of public view, and much of his remaining years were passed in the country at his house "Peterbrook," King's Heath, and at Llandudno with his life-long companion, his beloved wife. He sold his country house in the New Forest, with all its beautiful art treasures, and went to live at "Peterbrook."

This voluntary retirement at a comparatively early age has been made the subject of gross misrepresentation. Thus Doran says: "One of his biographers has observed that no great surgical position was ever made more rapidly, nor lost with such pathetic suddenness." Then Keen says: "Lawson Tait was undoubtedly a good surgeon and distinguished in his early life. But the reliability of his statistics was seriously questioned. He was much of an Ishmaelite in his profession, and the last years of his life closed in pitiable obscurity."

Now these statements about Tait's last years are misrepresentations. Keen, in his eagerness to discredit Tait because he persisted in his anti-vivisection views, paints his last years as ending in "pitiable obscurity"; while the other writer's affirmation that Tait's great surgical

position was lost with pathetic suddenness is simply ludicrous. Tait had made a name in the history of surgery that could never be lost; because he refrained from carrying on as active a practice, as in former years, did not warrant any one saying that he had lost his surgical position.

The real reason why Tait determined to take life more leisurely than formerly was that he was suffering from stone in the kidney; for this stone he had, in the end, to be operated on, and it was from his kidney affection that he died at a comparatively early age.

Before we outline the work of his last years, let us in a few sentences run over what he had already accomplished in the ten years between 1879 and 1889.

In 1879, Tait, in the words of Alban Doran, gloriously established hepatic surgery, for in that year he performed his first cholecystectomy. Next year, in 1880, he performed his first hepatotomy for hydatid cyst, and though he failed when given the opportunity to operate on a case of ruptured ectopic pregnancy, in 1881, he did not hesitate to do so in 1883. In the same year he brought out the *third* edition of his work on the "Diseases of the Ovaries." In 1884, he joined with others in founding the British Gynæcological Society, became its vice-president, and the same year visited America, where he gave lectures, and demonstrated his methods of work. In the following year, 1885, he published his paper on 139 consecutive ovariotomies *without a death;* and in 1886 he contested the Bardesley division of Birmingham as a Gladstonian, but was defeated by Mr. Jesse Collins. In the same year he was elected President of the British Gynæcological Society; while in 1888 he became Professor of Gynæcology in Queen's College, Birmingham, and during the same year delivered the Ingleby Lectures, choosing "Ectopic Pregnancy" as his subject, and these lectures he published in book form the same year.

In 1899, the first volume of his work on "Diseases of Women and Abdominal Surgery" was published, and during the same year he was elected President of the Birmingham Medical Institute, and a little later was made Principal of Mason College, Birmingham, and about the same period

he was awarded the Cullen and Liston Memorial Prize by the Edinburgh College of Physicians, for his service to medical science. The prize, which he left in the Art Gallery at Birmingham, consisted of seventeenth-century workmanship in silver.

During these eventful years he wrote many papers for medical journals, and read many papers before medical societies, and all the time he carried on a warfare about the operations that he was trying to force on the surgical world.

Then came a great event in his life, the delivery of the address on Surgery before the Medical Association at Birmingham, in 1890.

We now propose to outline his work during the remaining years of his life.

In the *British Medical Journal* (January 10, 1891), Tait writes a letter on "The Treatment of Uterine Myoma," in which he takes exception to something that Skein Keith had said about his handling of statistics; and in a later number (January 24) he has a long letter entitled, "Recent Debates on Gynæcology," in which he attacks Sir Spencer Wells. As this letter is a typical one we give it to illustrate his method of controversy.

Recent Debates on Gynæcology

Sir,—On p. 119 of the *British Medical Journal* of January 17, my friend, Professor Sinclair, is reported as saying: "The Staffordshire knot ought to become obsolete, for it was not to be trusted." Surely there must be some mistake. I have tied the knot certainly nearly four thousand times, and I know of its constant use by others with equal success when it is properly tied. Professor Sinclair must have meant that there were hands who could not be trusted to tie the Staffordshire knot. If it is tied according to the instructions I have given, its failure is an absolute impossibility.

On the same page Sir Spencer Wells alluded to a case operated upon here, and seen by him that day (January 7), and characterised by him as a "deplorable and disastrous failure. He had seen other cases almost as unsatisfactory." I confess I see nothing discreditable to the operator in one or two failures out of every hundred of such cases for there is not a single surgical operation which knows no failure. If there is anything discreditable in the present case, the discredit

must lie at Sir Spencer Wells' own door, and not at mine; for if the original account of the case be read, it will be seen that "during the last seven years, she had been the patient of a distinguished gynæcological baronet, who had, however, failed to relieve her. Ultimately she had been referred to him (Mr. Tait), and he had operated. She had double pyosalpinx of old standing, and it was very difficult to say which was tube and which was ovary. There were abscesses in both ovaries, and if he had attempted to tap them from the vagina, he would have been obliged to tap several cavities. Instead of doing anything of the kind, he opened the abdomen a month since, and the patient was now practically cured. A case like that was worth a dozen hypothetical imaginations. There was a woman who had been an invalid for years, who could have been relieved at any time, who had been under the care of all the well-known specialists of London, many of whom had declared that there was nothing the matter.

If anybody is to blame for the "deplorable and disastrous failure" it is Sir Spencer Wells, who, during seven long years treated this poor woman by pessaries, etc., instead of employing the only remedy possible—removal of the diseased organs. The reason, probably, why Sir Spencer did not do this I am prepared to argue, if he cares to prolong the discussion.

But the most interesting part of the case remains. Sir Spencer Wells saw the case on January 7 because of my absence in Italy. On my return I saw the patient on January 14. I take the liberty of controverting every word Sir Spencer Wells says about the case. It is not a failure at all. So far as the tubal and ovarian abscesses are concerned, the patient is not only "practically cured," but she is absolutely cured, for I have the diseased parts in a bottle.

The patient is now suffering from another condition altogether which Sir Spencer Wells cannot have discovered, or I am sure he would honestly have mentioned it, and not have fallen into the absurd mistake recorded as part of his speech at the Obstetrical Society. The whole story of this case is so interesting, and will prove so full of instruction, for others as well as Sir Spencer Wells, that I shall, in due season, give it in full detail.

I am, etc.

LAWSON TAIT.

At the Medical Society of London (January 26) he took part in a discussion on the operative treatment of chronic disease of the uterine appendages, and we find him defending with all his old vigour three things about which he never changed his mind.

He said :

> He was surprised to hear the President say that flushing did harm, for he was in the habit of using it constantly, and he had never had a mishap from it. The same remark applied to drainage. He found the drainage tube most valuable, especially in giving warning of the beginning of hæmorrhage. He kept it in a much shorter time than formerly—two or three days, instead of five or six. As to the Staffordshire knot, like other knots, it must be tied with care. Once or twice it had failed in his hands through not attending to his own directions for its use, but it was unfair to say that the knot could not be trusted.

On September 25, 1890, Tait delivered an address on the "Present Aspect of Antiseptic Surgery," [1] which was a very full criticism of Lister's address at the Berlin Medical Congress. Professor White, of Pennsylvania, replied at length (*British Medical Journal*, January 10, 1891), defending Lister, and, of course, Tait had to reply to White, which he did (*British Medical Journal*, February 14, and April 11, 1891). We shall only quote a few lines from one of Tait's letters to show that no argument from the other side had any effect in altering his views as regard germs. He says :

> The facts of the "beef and pickle argument" and those of the henwife and the housewife, are quite familiar to everybody in this country, and, taken together with those of the everyday practice of surgery, they prove three things : (1) that the germs of putrefaction exist, with restrained potentiality, in all living tissue ; (2) that the restraining influence is what we call, for want of a better name, vital force ; (3) that when the restraining influence has its balance upset by what we call disease, lowered vitality, accident, etc., the germs are let loose to work the evil with. This is the fundamental pathology which I was taught as a youth, and my mature experience confirms it every day in every particular.

At the January meeting of the Birmingham and Midland Counties Branch of the B.M.A., Tait read a paper on "Four Cases of Axial Rotation of Ovarian Tumours with subsequent Strangulation." One of these cases Tait mentions, was operated on by him when in Florence in December,

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[1] *Brit. Med. Journ.*, September 27, 1890.

1891. In one of the cases Tait made the observation that the rotation of the tumour was from before, backwards, and to the left, an opposite direction to that of any other case he had ever seen, but it was precisely the direction of twist that would be effected if the twist were due to the filling and emptying of the ascending colon.

In March (*British Medical Journal*, March 14, 1891) we find Tait writing a letter against the treatment of tumours by electricity. He says:

> Finally Dr. Keith writes: "Many years ago I had to do with an extreme case . . . where the tumour reached the enormous weight of 200 pounds." Many of your readers are familiar with my personal appearance, I weigh 210 pounds. I have just had a good long look at myself in a cheval glass, and have tried to imagine myself (less the odd 10 pounds) as a tumour in a woman. I once removed a soft œdematous myoma, which took two people to carry it to the scales, and this was found to weigh 70 pounds. A witty visitor made the comment, "You peeled the woman off that tumour very nicely." But 200 pounds! That must be a fishing story.

1891

Tait was elected Honorary President of the Obstetrical and Gynæcological Society of Glasgow, and he went to that city on February 2, 1891, to deliver an address, "On the Principle of Exploratory and Confirmatory Incisions."

The perusal of this address tells us much about some of those things which Tait thought essential in his surgical practice.

In the first part of this lecture he alludes to Robert Houston, who, in August 1701, extended the idea of tapping an ovarian tumour into that of an exploratory incision. Then Tait proceeds:

> The interest for us is that the story of the case, passing through William and John Hunter, reached John Bell, whose pupil Ephraim McDowell was in the year 1793. John Bell alluded to the probability of ovarian tumours being some day successfully removed, McDowell carried out the idea. The story of his first experience he sent in manuscript to his old master, John Bell, in the year 1816; but John Bell was

then upon his death bed in Rome, where I visited his grave but a few weeks ago, and the manuscript fell into the hands of Lizars who was at that time doing Bell's work, and this accounts for the fact that Lizars was the first to follow McDowell's footsteps on this side of the Atlantic.

Tait then goes on to point out that little was attempted in abdominal surgery up to 1872 except the removal of ovarian tumours; he is not quite just to the French, who were doing much to help the operative treatment of fibroid tumours by removing them by the extra-peritoneal method. He then says:

> Between the years 1872 and 1878 a revolt occurred in various directions. It was sought to establish, for the work of abdominal surgery, a duty far higher than merely saving half-dead women from the grave—a duty of relieving suffering as well as of saving life. And with this higher object in view, the new school of gynæcologists claimed the right of removing tumours long before they threatened life; claiming the right of removing diseases which did not threaten life at all, but made it a burden; claimed the right of interfering in cases where there was great doubt as to the very nature of the disease, and after a great deal of struggling in the dark this idea became crystallised. In the year 1875 I formulated a proposal quite new in the department of surgery, to the effect that we were entitled to open the abdomen in all cases where there was disease present which threatened life, or made it a burden, for the purpose of seeing in which direction the diagnosis could be made more certain, and how the line of effectual treatment could be opened up. . . . The new principle introduced, and certainly alluded to on the other side of the Atlantic, as "Tait's Law," was first of all subject to a great deal of misrepresentation, and even within the last few years I have heard with my own ears the president of a distinguished London society say that "Tait's Law" was that when you are in doubt you must open the abdomen.

Tait then says that he heard Huxley say that he thought that all men of science ought to be painlessly extinguished at the age of fifty-five; the reason given was that the old school often would not adopt new views and so hampered the younger man. Tait then goes on to say: "In gynæcology we are going through exactly the same kind of struggle; we are hampered and watched and vexed perpetually by

elderly persons, who ape a juvenility which they have long since lost and a wisdom which they never possessed." [1]

Needless to say that Tait's remarks referred to Sir Spencer Wells and to Mathews Duncan, and he proceeds to give his estimate of Duncan and to relate how the latter was forced to attend one of his abdominal sections when Tait removed "two huge suppurating ovaries and tubes," and this showed Duncan how "grotesquely he had blundered, and how terribly the patient had suffered by his obstinate refusal to the voice of progress." But the lesson was lost on Duncan.

Tait then devotes the remainder of the lecture to a full explanation of the "Principle of Exploration," and the difference between the *exploratory* and the *confirmatory* incision.

He said that the meaning of an *exploratory* incision must be perfectly appreciated before one considered the confirmatory incision—and he explains that an exploratory incision is one in which, having done your best to make an exact diagnosis, you have honestly to say that you hesitate between two conditions—say, for instance, a solid ovarian tumour and a soft œdematous myoma. As you are not certain you must confess that you are forced to make an exploratory incision to clear up the doubt. As the surgeon gains experience he will make less explorations, and Tait remarks, "I know that in my own cases I do not make one now for eight or ten that I made some years ago."

> Confirmatory incision, he says, on the other hand, became more common with me because of the same reason, that the exploratory incisions are fewer, and are limited to a class of cases where the majority suffer from hopeless disease. . . . In all cases, therefore, where a conviction is forced upon me that the disease is malignant, I tell the patient so, but at the same time say that all such opinions are liable to error, and that a confirmatory incision is the only and best way to give security.

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[1] Charles Darwin, said: "What a good thing it would be if every scientific man were to die when sixty years old."

At the annual meeting of the British Medical Association, held at Bournemouth during the first week in July, Tait read a paper on the "Treatment of Hernia by Median Abdominal Section."

Tait, in this paper, proposed to open the abdomen in all cases of hernia in which operation is required either for the relief of strangulation, with an operation for the radial cure superadded, or merely for the cure of a chronic hernia.

He pointed out that this was no new idea of his because as far back as 1883 he had first brought forward the proposal.

The proposal was not received with much favour. Operators admitted that the procedure would be of use in certain cases, but absolutely refused to accept the plan as a routine treatment for all kinds of hernia. The chief arguments urged by most operators was that in cases of strangulated hernia the obstacles to their reduction were chiefly placed in the sac of the hernia itself, and these obstacles could not possibly be dealt with from within the abdomen. There was also danger of tearing the softened bowel walls by traction on the gut, and there was considerable danger of introducing into the general peritoneal cavity some of the infected fluid contents of the sac, as well as the danger of dragging a gangrenous bowel through the abdominal contents to bring it up to the abdominal incision.

This proposal of Tait has never become popular, but it must be admitted that there are rare occasions when it is of great assistance, when combined with the usual incisions for the radical cure of femoral or inguinal hernia. It is quite necessary to use it when dealing with examples of obturator hernia. In one case of femoral hernia, where much bowel and omentum was in the femoral sac, we were very glad to open the abdomen and with one hand inside and one in the sac reduce the huge mass.

During the latter part of 1891, he carried on the war against antiseptics in a series of letters to the *Lancet*. It will not be necessary to quote these in full; we shall give only a few sentences from some of the letters.

Dr. Charles Tayler, of Derby, says in one letter (October 17, 1891) :

> Mr. Lawson Tait, in his answer to Mr. Adamson, admits that he gave up general surgery twenty years ago, and from his repeated utterances in this quarter one is inclined to infer that he gave up pathology at the same time. May I, a pupil of the founder of modern surgery, assure Mr. Tait that antiseptics do not mean the copious use of poisonous solutions, nor the complicated application of many dressings, but the dealing with each case as its needs are, approaching it with the caution of one about to perform a chemico-biological experiment, and with a thorough grasp of the facts of modern pathology.

In the first part of the letter Dr. Taylor had admitted that there had been two deaths from septicæmia at the Derby Infirmary, and in them the treatment had been on antiseptic principles though, unfortunately, in the two fatal cases, not very strictly applied. Tait in his reply seizes on this admission and uses it in the following way (the *Lancet*, October 24) :

> He says that during the last two years there have been only two deaths from what he calls "septicæmia" in the Derby Infirmary, and in these the antiseptic treatment had not been very strictly applied. The assumption is, therefore, that if the antiseptic treatment had been strictly applied these deaths would not have occurred ; certainly the logical conclusion is that they ought not to have occurred. This means that some person has been guilty, according to Mr. Taylor's facts, of criminal neglect. There ought to have been two inquests over these two cases and two committals for manslaughter if Mr. Taylor's conclusions can be logically maintained.

In the last part of the letter Tait points out that £70,000 is now being asked for to rebuild the Derby Infirmary :

> If Mr. Taylor's views and statements are correct, and the simple application of some method of antiseptics known to him, and having such marvellous results, can exclude the surgical scourge now going by the name of "septicæmia," there can be no need of a new infirmary. If, on the other hand, there is need of a new infirmary, Mr. Taylor's statements cannot be relied upon, and I leave him to settle which horn of the dilemma is least disagreeable to him.

In the *Lancet* (January 2, 1892) Tait writes a letter about the corpus luteum, and says that the conclusions that he has long come to is, that the belief that the corpus luteum is altered in any way by pregnancy, or that there is such a thing as a true corpus luteum or a false luteum in relation to the fact of pregnancy, is one of the most extraordinary crazes which has crept into medical belief. Tait's views on this point have been accepted, and it is now generally admitted that the distinction between the two—the true and the false—is based entirely on their relative size; they both present the same structure, the large size of the true corpus being no doubt simply due to the increased vascular supply which is a concomitant of pregnancy.

Towards the end of this year Tait read a paper, at the Royal Medical and Chirurgical Society, "On the Association of Pleural Effusion with Disease of the Abdomen" (November 24, 1891).

He said that the object of the paper was to draw attention to the occurrence of pleural effusion as a condition occasionally associated with disease of the abdomen. In some cases the abdominal disease was malignant, and he had formed the opinion that, if the pleural effusion proved to be of a bloody character, the surgeon should decline to interfere, as the effusion indicated malignancy. He related, however, a case to show that this was not to be regarded as a fixed rule, for after several tappings of the pleura had been carried out he consented to operate and found that the abdominal condition was a fibroid tumour of the ovary, the removal of which led to the patient's recovery. Tait took this occasion to say that the above case confirmed him in his rule that all cases of abdominal disease, however unfavourable the aspect of the case was, ought to have the opportunity of an exploratory or confirmatory incision.

At the February (1892) meeting of the British Gynæcological Society, Heywood Smith read a paper on "Subperitoneal Hysterectomy." In this paper the writer said that if it were possible to carry out a technique whereby the vessels could be effectually secured, and at the same time to do away with the disadvantages of the clamp, he thought

that the operation cases would get well more rapidly, and that the patients would be less exposed to danger from septic absorption. Heywood Smith brought forward much evidence in favour of the intra-peritoneal treatment of the stump, and we must regard his paper as one of the first and most important papers published in England on the subject of intra-peritoneal treatment of the stump after abdominal hysterectomy ; and it is to the credit of the author that he saw that that method was the proper one, and the one for the future.

Greig Smith, the vice-president of the Society, was in the chair, and Tait was present. Both spoke, but neither was in favour of Heywood Smith's views, and it was during the course of the debate that Tait said that, " short of some altogether fresh light upon the subject he intended in future to adhere to the extra-peritoneal method."

At the March (1892) meeting of the British Gynæcological Society, Tait read " A note on the alleged occurrence of Tubal Abortion." In this paper he throws doubt on Bland Sutton's observations, and objects to the introduction of the term " Tubal Abortion." Tait said :

> I have now operated on sixty-three cases of tubal gestation soon after the time of rupture. I have seen over twenty *post-mortem* examinations, and one way and another I must have examined more than a hundred specimens, and I am sure out of this number of nearly two hundred examples I never saw a single instance where the hæmorrhage had passed through the abdominal ostium.

We now know that Tait was wrong, and that all observers agree that " Tubal Abortion " is by far the most frequent end of tubal pregnancy. Tait's view was supported by Werth, who, in 1892, stated that he could only find " six cases of tubal abortion reported in over 600 cases of tubal pregnancy." Twelve years later, in 1904, Schrenck reported that an examination of specimens showed that seven out of eight cases end as tubal abortion, while one ends by rupture. Tait, we know, was obsessed by the idea that the fate of tubal pregnancy was either rupture into the peritoneal cavity or

rupture into the folds of the broad ligament. He objected to the introduction of the term " tubal abortion " because, if such cases did occur, they presented neither clinical nor pathological features by which they could be classed apart from cases of tubal rupture, nor did the surgical necessities of the two conditions differ in any way. As regards the latter observation we have shown that Tait was even wrong in that, as tubal abortion is not nearly so serious a condition as a rupture of the tube at the isthmus.

At the June meeting of the Royal Medical and Chirurgical Society Tait read a paper on the " Alleged occurrence of Ovarian Pregnancy " ; the object of the paper was to explain those cases, where in the wall of a gestation sac true ovarian tissue had been discovered, and at the same time the apparent absence of the ovary, otherwise than as constituting the sac of the corresponding side. Tait maintained that there was no specimen in existence that could demonstrate clearly the occurrence of ovarian pregnancy. This has now been done in several specimens operated on in the last twenty years.

At the June (1892) meeting of the Obstetrical Society of London, Tait read a paper on " A case of Ectopic Pregnancy in which the fœtus seems to have been developed to the full time in the peritoneal cavity, still retaining its amniotic covering."

This case, as Doran remarks, " bids fair to become classical," for it was the first case in which it had been shown, " that a living fœtus, practically not more than ten weeks of age, is capable of resisting the digestive powers of the peritoneal cavity if the amnion is unbroken."

Very soon after Tait published his case, Clarence Webster published an illustrated work on a similar case, and later on Taylor, of Birmingham, gave a full description of a like case that had occurred in his practice, and brought forward evidence to show that these cases rupture into the peritoneal cavity, not into the folds of the broad ligment and then into the peritoneal cavity.

At the same meeting of the Obstetrical Society, Tait and Christopher Martin, his partner, read a " Note on the

Growth of the Placenta after Death of the Fœtus in Ectopic Gestation."

In May, 1892, Tait had to defend an action brought against him by another medical man : " Denholm *v.* Tait." [1] The case was tried at the Manchester Assizes. The action was one for libel.

The patient had a uterine myoma, and had undergone a course of the Apostolic electrical treatment under Dr. Denholm. In the course of the treatment an accident had occurred which gave rise to a vesical fistula. The patient then consulted Tait, who blamed the electric treatment and advised removal of the fibroid tumour. The operation was performed. Soon after the operation the husband called, and was told the patient was going on splendidly, but within forty-eight hours he got a telegram summoning him at once to his wife. Half-an-hour after he arrived she died. The husband was anxious to see Mr. Tait, but was unable to do so. The following letter, which contained the alleged libel, was handed to him :

> My dear Sir,—I saw your wife, with Mr. Christopher Martin, between two and three times this morning, and matters were going on very fairly with her. At 7.30 I was summoned again, and found her sinking. You were summoned at once, and, I am glad to learn, reached her bedside before she died. The immediate cause of this unfortunate result is the rupture of a blood vessel at or near the spot where the electrical needles caused so much damaging inflammation and sloughing. Had she never been submitted to that treatment the case would have been a straightforward one, and her recovery almost certain.

The husband considered that the letter imputed the death of his wife to improper treatment before the operation, and went to see Dr. Denholm, who advised a *post-mortem* to find out the true cause of death. This was carried out, and it was shown that death was due to peritonitis.

It was, however, shown at the trial that the *post-mortem* examination had been made by gaslight while the patient was still in the coffin, and that a hæmatocele of the broad ligaments had been overlooked, while the evidence of peritonitis was very slight.

[1] *Brit. Med. Journ*, April 2, 1892, p. 739.

Tait was called and gave his evidence, and then Mr. Gully, on behalf of the plaintiff, and without calling the witnesses of Mr. Tait, said that having heard the statement of the defendant, his client, in the fullest manner, withdrew the imputation that Mr. Tait had wilfully or recklessly misstated the cause of death; and Mr. Walter Whitehead who had stated in the box that in his opinion Mr. Tait had recklessly operated on the patient, now having heard Mr. Tait, wished to withdraw the statement.

The judge said that he could not help feeling that if the parties had had the opportunity of meeting and knowing the facts of the case on both sides, these difficulties would never have arisen between them.

During 1892, Tait communicated letters and papers to the *Lancet* on the following subjects:

The Corpus Luteum; Uterine Dilation; The Royal College of Surgeons; Alleged Occurrences of Ovarian Pregnancy; On the Surgery of the Liver and Gall-Bladder; Experiments on Living Animals; Taking the Oath.

To the *British Medical Journal* he contributed the following:

The Medical Defence Union; The Modern Treatment of Uterine Myoma; The Alleged Treatment of Tubal Abortion; The Council of the Royal College of Surgeons; The Alleged Occurrence of Ovarian Pregnancy; Ectopic Pregnancy; Hysterectomy; Growth of the Placenta after Death of the Fœtus in Ectopic Gestation; Female Inebriety; On the Abolition of Vivisection and Anti-vivisection; Peritonitis, its Nature and Prevention; Treatment of Peritonitis and the Scientific Grants Committee.

Tait offered himself for election at the Royal College of Surgeons, as he had done in 1889, and 1890. He belonged to a reform party and he wanted the provincial fellows represented on the council.

The *Lancet* (July 11, 1891), in commenting on the elections, says:

> In the elections just concluded, the dominant idea in the official mind was to keep out Mr. Lawson Tait. . . . Mr. Jessop's candidature was necessary in order to keep out the

arch enemy, Mr. Lawson Tait, and this has certainly been effected, but in all probability it will be found that a very good reformer has been seated on the council, and that Mr. Lawson Tait will be returned in a very short time. It must be remembered that the latter gentleman was junior to all other fellows engaged in the contest, and that he polled more votes in 1891 than he did in either 1890 or 1889.

In the *Lancet* (September 12) Tait published a "Clinical Lecture on the Details necessary in the Performance of Abdominal Sections," and tells us that this lecture was delivered at Queen's College, Birmingham, as part of his course of lectures, he being at this period Professor of Gynæcology.

We may take a sentence here and there to illustrate some characteristic points in Tait's surgical methods.

(1) The patient upon an operating table, which is simply a deal board resting upon two trestles; as I deprecate any kind of complicated apparatus, I deprecate any kind of complication in the table upon which I am to perform the operations.

(2) Again, I say that in all my instruments two things have to be regarded; first of all, scrupulous simplicity, and, secondly, scrupulous cleanliness.

(3) We, who are opposed to the antiseptic doctrines and the antiseptic practice, argued that the germs were harmless if dead and dying material upon which they might feed were removed from the wound. The washing out of the abdomen, therefore, for the careful removal of all clots, dirt, and debris, and the free use of the drainage-tube, form the complementary practice of essential cleanliness, and pave the way to our recent enormous advance in abdominal surgery.

(4) Simplicity of apparatus is the end to which I have ceaselessly striven, and you will see, therefore, that my whole armamentarium of weapons consists of simple little knives, simple catch-forceps, a simple cannular trocar, and, for the special operation of hysterectomy, a simple wire clamp. You can carry the whole set of instruments in one coat pocket.

(5) Tait then describes minutely the way the abdominal incision should be made, and the manner in which hæmorrhage from the vessels is to be checked, and tells how the wound is to be closed. He then alludes to the dressings for the wound:

> Your dressing need consist only of a piece of unmedicated dry cotton-wool ; or if you adopt, as I have done for the last few weeks with one advantage, an additional agent for drying your wound and keeping it dry (for that is the best of all forms of true antiseptics), the powder of boracic acid will be found the most satisfactory drying agent for any kind of wound.

The greater part of the leading article in the *Lancet* of November 26, 1892, is devoted to anti-vivisection and Tait. At this period a very heated controversy was being carried on in the *Times* in regard to vivisection and Tait had taken an active part in this correspondence. The *Lancet* points out that the position taken up by some of the anti-vivisectionists was that under no circumstance is experimentation on animals justifiable. The editor, however, admits that Tait to his credit has accepted that the question to be determined is : How much good has been done, and how much information has been obtained, by experiments on living animals ? The article draws attention to the fact that Tait maintained that " no great advance in medicine or surgery has been made which could not have been gained by clinical observation."

Tait replied in a long letter (*Lancet*, December 3, 1892), and his reply will show how hopeless it was to try and convince him that his views were wrong. Here are his words :

> Similarly I might assure you, that in my department the labours of Pasteur and Lister had not only nothing to do with my success, but, on the contrary, my success only came to me when I had completely disregarded the entanglements into which their teachings had led me. Since I left their following I have ceaselessly been engaged in combating their mistaken application of the facts relating to dead tissue to the treatment of the living.

1892

In the *Lancet* (November 12, 1892) appears a letter from Tait on " Experiments upon Animals." A few sentences in the letter show us clearly that Tait had evidently made up his mind not to practice some of the operations which were being done at that time—operations which were really

the most advanced things in abdominal surgery. The following from his letter clearly shows his attitude and views on these procedures:

> In whatever part of the intestinal canal it is practised, "pylorectomy" has nothing, therefore, to single it out as a case where experiments on animals are required more than in other parts of the intestines. But if Mr. Jessett wishes to claim for it such a character I will grant his premise at once, because it is one of the class of operations which I utterly condemn. I have never done it, and I never shall, for the disease is malignant nine hundred and ninety-nine times in a thousand, and inevitably returns. . . . We now know also that such operations as gastro-enterostomy lead to permanent, continuous fæcal regurgitation, so that the whole scheme of such operations is gradually being dropped.

Tait attended the meeting of the British Medical Association held at Nottingham, and spoke on the paper contributed by Mayo Robson, who had been one of the surgeons who had come to Birmingham and had studied Tait's methods. Robson's paper was on the Surgery of the Liver and Gall-Bladder. Tait made some remarks which are further evidence that he did not propose to advance into those surgical fields that were open to all, for in his remarks on Mayo Robson's paper he said: "Resection for cancer had no sympathy from him. . . . Gall-stones do not form again after operation, and cholecystectomy is therefore rarely necessary."

At this meeting Tait presented a table of cases which showed twenty-one exploratory incisions with four deaths, eighteen hepatotomies with two deaths, and seventy-one cholecystotomies with four deaths.

During this year Cullingworth, an ardent follower of Tait, who had transferred from Manchester to London, read a paper before the Obstetrical Society of London on "The Value of Abdominal Section in Certain Cases of Pelvic Peritonitis, based on a Personal Experience of Fifty Cases."

This paper is excellent in every way, and was one of the first clear expositions of the value of abdominal sections in pelvic inflammation, read in London by a follower of Tait. It was discussed by John Williams, Alban Doran,

Champneys, and Playfair. It shows how obstinate the older practitioners were to Tait's new views. One has only to quote the words of Playfair. He said :

> He would object altogether, for example, to the acceptance of Dr. Cullingworth's third proposition where distinct swellings are found in the posterior quarter of the pelvis in connection with recurrent attacks of pelvic inflammation, surgical relief is usually indicated, and, generally speaking, the sooner such relief is afforded the better. Who was there who had sufficient long experience of more conservative practice, who could not call to mind case after case of recurrent pelvic peritonitis accompanied by complete fixation of the uterus, with distinct swellings in the posterior quarter of the pelvis, which did not eventually recover without surgical interference of any kind . . . ? So far from admitting that such case should be interfered with soon rather than late, he believed that removal of diseased appendages should be considered a *dernier resort*, instead of a *premier resort*, and should be looked upon as a confession of failure of cure. These cases rarely proved fatal *per se ;* doubtless they led to a vast amount of pain, suffering, and broken health, which very often fully justified operating, but they could generally wait until they were sure that nature could not effect a cure.

If men who held the high position that Williams, Wells, and Playfair did, still persisted in their opposition to the removal of diseased appendages, it is little wonder that the great bulk of the profession hesitated to adopt Tait's views. Yet this makes Tait's position as a pioneer in this branch of surgery all the more remarkable, and all the more creditable.

In November Tait published, in the *British Medical Journal* (November 12, 1893), a paper at the Harveian Society of London on this subject, and this paper contained his main arguments.

This is a very important communication, because it gives Tait's matured views on many points upon which he laid great stress. We propose to quote from the paper later on when dealing with the personal aspects of Tait's work, particularly in reference to his after-treatment of section cases.

In the same number of the *British Medical Journal*, as

Tait's paper appeared, there was published the *Bradshaw Lecture* on "The Signs of Acute Peritoneal Diseases," by Samuel Gee.

Let us read—as a contribution to the history of the surgery of the abdomen—what Gee says as regards "Incision for the Purpose of Diagnosis."

> A small incision would seem to be justified by conditions such as these: First, the patient is suffering from acute peritonitis and the indications of poisonous infection of the blood are becoming more and more marked; or next there are reasons for believing that perforation of the peritoneum has occurred. In either case unless we can bring relief, the patient must die. A small incision will not make him worse: nay, to drain the peritoneal cavity affords a chance of recovery; the only objection lies in the natural dislike to an operation, however small. However, suppose a small incision is made and we find that the peritoneum contains pus or air, beyond simple drainage two further courses lie open to us; we may follow John Hunter's suggestion, and wash the belly out with warm water, a proceeding more serious than simple drainage, or we may enlarge the incision and search for the cause of the peritonitis, a proceeding highly dangerous, for you will, I think, agree with me when I say that operations of this kind have seldom any other effect than to hasten the patient's end.

The above quotation is from Samuel Gee, one of England's wisest clinical teachers, and it shows us only too clearly how hard it was for the mind of the physician to grasp what the surgeon was trying to tell him. "Explore and wash out," cried the surgeon. "Do so, if you will," replied the physician, "but death will follow."

1893

To the *Lancet* (vol. i., 1893) Tait contributed some papers, the chief one being on "Sensitiveness of the Peritoneum." Tait himself believed that the peritoneum was most sensitive, but did not distinguish between the parietal and visceral peritoneum. He tried to obtain the opinions of many well-known surgeons, and he published their replies (the *Lancet*, January 21, 1893).

In the May (27) number is a description of a "Simple

Method of Performing Supra-pubic Cystotomy." His description of the operation shows us that he did the operation in the same way as we all do it now. We do not know whether or not he was the first to describe this excellent method of operating; if he was, then it shows how far-seeing his surgical instinct was, even to the use of the glass drainage tube which we have found much superior to the rubber tube in this locality.

In the November (18) number of the *Lancet* there appears a letter from Tait on "A Case of Nephrorrhaphy."

The letter shows us that Tait was always opposed to the operation of Nephrorrhaphy; he says: "I have narrowly watched the rise and decline of this operation, because it is one which I distrusted from the first." After thirty years we all seem to have come to the same conclusion.

He then related that an experienced American physician was sent to him from America and he was requested to fix his kidney. He refused to do so, but the patient begged Tait to make an exploratory incision, which he did, but he did not fix the kidney. The patient returned home and was operated on for "movable kidney." He died soon after; a *post-mortem* examination showed that Tait was right in his refusal to fix the kidney, for the case confirmed what he had maintained for years: "That I have never met with a movable kidney where mobility was the only pathological condition; the *post-mortem* revealed the fact that the patient had pyothorax, pulmonary tubercle, and caries of five dorsal vertebræ (eight to twelfth)."

Tait read a paper at the January meeting of the Birmingham and Midland branch of the B.M.A. on "Ectopic Gestation." In this paper he merely went over old ground affirming that all ectopic pregnancies were primarily tubal; that normal adhesion of an impregnated ovum takes place in the uterus only; and that impregnation never occurs in the tube save after more or less complete desquamation of its ciliary coat. This paper shows that Tait had not changed his mind on these points; all of these points are now disputed.

In the February (25) number of the *British Medical Journal*, Spencer Wells continues the vendetta against

Tait by publishing a paper entitled, " The End of a Practical Cure."

The paper is illustrated by a full-page chromo-lithograph, and Wells related how, as far back as 1879, he had amputated the cervix uteri of a case which until 1888 remained well. She then became ill and went to Birmingham, and " she was there assured that her life was in imminent danger, that immediate operation was necessary, and that she would die or become insane if it were not done, that the risk was very small, the after-results insignificant, and a cure almost certain. Her ovaries and Fallopian tubes were removed."

Wells stated that after the operation the patient was more or less a confirmed invalid and suffered very much from menorrhagia, and ultimately was operated on by Professor Ceci, in 1892, who performed a vaginal hysterectomy, and the patient died. The uterus was forwarded to Wells who had it examined, and it was then discovered that it contained a small submucous fibroid which caused the menorrhagia.

Tait, of course, had to reply to this attack, which was actuated by the lifelong feud between Wells and himself. What Tait had done was quite proper ; he had removed two pus tubes, the result of gonorrhœa, and these tubes Wells had missed—it was, however, a piece of bad luck for Tait that a small submucous fibroid should cause so much hæmorrhage that the uterus had to be removed. The concluding sentence of his letter is a good example of his method of giving an antagonist a parting shot ; he says :

> Vaginal hysterectomy is a simple and easy operation, but it is rather a heroic remedy for a small submucous myoma and chronic intra-uterine catarrh, especially when its ending is, as described in a letter now before us, " There was wounding of intestine—the usual consequence, death " ; a circumstance not alluded to by Sir Spencer Wells.

The letters have but little interest beyond showing that, as far as the history of the operations for removal of pus tubes is concerned, in the year 1893 Spencer Wells—one of the leaders of surgical thought in those days—wrote :

> Afterwards, when delivering the Bradshaw lecture at the College of Surgeons, in December, 1890, I expressed my fear

that "the operation of oophorectomy has been injudiciously performed without due explanation of its consequences, and with mistaken prophecies of insanity or early death; that in certain circumstances the bounds generally recognised as those which should limit deliberate and consultative practice have been over-stepped; and that many young women, who have been saved from unnecessary mutilation, have afterwards borne children . . . all this has excited a suspicion in the public mind—a more than a suspicion in the mind of the profession—that some of the recent expansions of abdominal surgery have not increased public respect for the profession, and require denunciation."

In a way Spencer Wells was right. There can be little doubt that many normal ovaries and tubes were removed in those days of experiment and groping; but while admitting this, we cannot overlook the fact that Wells would not acknowledge the great benefit that Tait had conferred on humanity by his operation on pus tubes. Spencer Wells was blinded by jealousy, and he refused to acknowledge that pus tubes were pathological conditions of much surgical importance. Herein was his great mistake—he was too old to learn, and too jealous of his younger rival to be just in his criticism.

In the November (1893) number of the *British Medical Journal* Tait writes to say that the paper, contributed by Walter Heape to the *Proceedings of the Royal Society*, on "Menstruation of *Semnopithecus entellus*" confirms the views that he had held for years. He says:

> Out of the ovaries of forty-two specimens of menstruating *S. entellus*, only two were found in which recent discharged follicles were seen. Such a result appears to be sufficient to warrant the statement that, (1) ovulation does not necessarily occur during each menstrual period, and (2) that menstruation is not brought about by ovulation: the facts brought to light by Heape completely confirmed those to which Tait had already directed attention, and their conclusions were identical.

In the January (13) *Lancet*, Tait contributes some notes on a case which was of considerable interest to him; he says:

> The interest of the case lies in the circumstance that, as one of interstitial tubal pregnancy, it is of great rarity, and it

constitutes the first instance in which surgical assistance has anticipated the rupture, and the patient's life has been saved. I have said in my lectures on Ectopic Pregnancy that I thought it very unlikely that any one would ever have such an opportunity.

In the June (2) number of the *Lancet*, Tait said that the representatives of the late Charles Clay, of Manchester, had placed at his disposal a mass of material for the preparation of a suitable memorial of him, including an autobiographical sketch. He writes to ask if any one is in possession of documents, or recollections, of Charles Clay, as he would like to communicate with him.

In the November (10) number of the *Lancet* appears a letter which shows us that Tait was not ashamed to own up to his failures, and it also shows us that he was ready to champion the cause of a younger surgeon :

THE WOMEN'S FREE HOSPITAL, SOUTHAMPTON

To the Editors of the *Lancet*

SIRS,—You surely are in error when you say that it is a new departure that an institution such as this should have members of its consulting staff living at remote distances from the spot at which their services are likely to be required ? I have looked casually over the list of hospitals in Churchill's Directory and have found at least forty examples of this relation. I am on the consulting staff of six institutions where this relation exists, and in at least one of them it has dated over twenty years. I have often been summoned to these institutions to give my services and have never missed doing so. In the case of the Southampton Hospital, during the year 1893, I was summoned to give my advice in eighteen out of the twenty-four cases, and I attended all these summonses. In four of the cases there were such evident difficulties in the way that I undertook the operations at Mr. Elliot's request. Of these, I regret to say, two died. One other death occurred from the advance of malignant disease after an exploratory operation, which exploratory operation had nothing to do with the death of the patient. This leaves Mr. Elliot's mortality as one in nineteen, a result by no means discouraging for a beginner. I wish I had had it twenty-five years ago, or anything approaching it. If Mr. Elliot could have found any one in Southampton likely to help him with the experience such as I can place at his disposal I do not suppose any one outside Southampton would have been asked to take the

place and responsibility I willingly accepted. Southampton is a place large enough, and of sufficient importance, to be a local centre of abdominal surgery. Mr. Elliot is a brilliant, conscientious young surgeon, and it is alike a pride and a pleasure to me to help him on his way, and I am sure you will not say, after the facts I have given above, that I have taken an office of a purely ornamental character.

I am, Sirs, yours faithfully,

LAWSON TAIT.

The Crescent, Birmingham.

In the May (4) number of the *Lancet* Tait shows how he devoted much time to the small hospital at Southampton. One reason for his interest was that his country house was in the New Forest, not far away from Southampton :

When I first began to know Southampton intimately from residence in its neighbourhood, I was asked to see a number of patients in consultation, and to do a number of operations. I was not surprised to find a large proportion of cases of bad pyosalpinx, for Southampton is just the kind of place where I should have expected to find them. But I was surprised to find that no effort whatever had been made effectually to treat these cases as they must be treated, and I could not discover that any ever had been treated at the Royal South Hants Infirmary. I urged the establishment of a special hospital first on Mr. J. F. Buller, who had been one of my post-graduate pupils, and then upon Mr. Elliot. The former gentleman elected to follow another special line of practice by opening an ophthalmic hospital. Mr. Elliot followed my advice, and I took no small pains to do all I could to help him towards success, and I am glad to feel that I succeeded. I operated on a large number of cases, some in Southampton, many more in Birmingham. If all these had been included in Mr. Elliot's list their number in ratio might have, indeed, seemed to be "appalling," but they were merely the accumulation of the neglect of years. I attended meetings of the Southampton Medical Society, I showed specimens of Fallopian bags of pus fresh from Southampton patients, I did my best to instruct my audience in the diagnosis and treatment of such cases ; some if not the whole of the surgical staff of the infirmary were present, but up to the present day it would seem as if gonorrhœa and its products were unknown in Southampton so far as the infirmary is concerned, for not a case of damaged appendages is yet recorded as having been dealt with there.

In the August (10) number of the *Lancet*, Tait returns to a subject that he had been interested in for many years :

"Perforation of Uterus by Sound or Curette." A few sentences from these papers are of interest. He says: "I have seen as many pathological tubes as I suppose any other living person, but I never saw a tube in which abnormal patency was a feature."

Another sentence shows us that Tait occasionally did vaginal hysterectomy, for he says: "I have also seen two uteri through which the sound or curette actually had been passed, one at the time of an ovariotomy, and the other recently in a uterus I removed by vaginal hysterectomy."

He says: "I have also recorded three cases of what appeared to be cases of permanent metro-peritoneal fistula. One of these cases possesses the fistula still, and I know the patient is still alive, as I passed a sound at the old spot three years ago."

In the February (1) 1896 number of the *Lancet*, Tait published a letter from an American medical man who had been committed for murder, on account of his treatment of a case. Tait published his reply to the medical man and says that he believes that it was an instance of "metro-peritoneal fistula," and ends his letter with the following words: "Instead of trying you for murder your prosecutors ought to congratulate you on your skill and good judgment. If time permitted I would willingly appear in the witness-box to sustain this, my written opinion. Make any use of this you think fit."

In the May (16) number of the *Lancet* Tait published what was probably one of his last important papers. The subject was, "The Evolution of the Surgical Treatment of the Broad Ligament Pedicle."

In this paper Tait tells us why he has not written the second volume of his works on "Diseases of Women." He was looking for some new method of treating the pedicle, because he had formed the opinion that the ligature caused, or facilitated, the occurrence of broad ligament effusions, under which heading of course was broad ligament hæmatocele:

> The second incident which tells against the ligature is the occurrence of a number of cases, 3 or 4 per cent., in which the garotted stump and ligature get the better apparently of

> the living tissue around them, prevent the tissues absorbing them, cause suppuration, form sinuses which do not heal, but go on discharging till the knot of thread is got rid of, months, it may be years, after the original operation. Sometimes they burrow into sinuses of the broad ligament and are discharged into the rectum and bladder. . . . How these ligatures travel in this way (into the bladder) is a matter of easy explanation, and that is, I think, as follows. Suppose the ligature is placed in the right pedicle it will go (as a matter of fact, three of my own have so travelled) into the bladder; while if it be a left broad ligament pedicle to which the ligature has been applied it will more likely get into the rectum. In fact, as I have often had occasion to observe on operating for pelvic abscesses, the bladder seems to be the road out of the right broad ligament, and unquestionably the rectum is the point of election for the natural evacuation of the left parametric cavity.

Tait then gives a series of diagrams to show how the silk ligature is enclosed by the bladder wall and gradually suppurated into the bladder.

These are two of the reasons, he says, why he wants to get away from the ligature, and after years of experiment and thought he thinks he has at last succeeded, and proposes a plan which is to be worked out by "cooking" the pedicle. He says:

> Keith did not sear the stump. It is true that he burnt a piece of it off after securing it with Baker Brown's clamp, and if searing had been the means of his success he would have stopped there. But he went on for about twenty minutes or half an hour rubbing the clamp with the cautery and cleaning it with a towel or sponge. . . . What he had done was really this . . . the enclosed stump of pedicle was cooked dry like a strip of parchment.

Tait said that he himself had set out to find a method of cooking the pedicle, and at last succeeded in doing so by means of electricity:

> Knowing the resistance of a certain piece of platinum wire I embedded it in a box of silver and isolated it by means of plaster of Paris, Kaolin, or pipe-clay. Two such boxes are placed face to face and connected by the necessary binding screws. The boxes are placed opposite each other on a pedicle and are screwed together. An electric current of known strength, and under the control of a rheostat, or a Kelvin's

> balance, is now turned on to heat the boxes, or, better still, the boxes are heated first and then applied to the pedicle, as this method saves time. The boxes are enclosed in ivory or some other bad conductor of heat, to save alike the heat and the tissues for which the heat is not wanted, though this precaution may be dispensed with. The apparatus that I now describe will so cook an ordinary pedicle that hæmorrhage will be impossible and suppuration unlikely in the extremest degree in a space of about six to eight minutes.

In a later number of the *Lancet* (May 23, 1896), Tait writes to say that, " we have now applied the instrument to seventeen pedicles with uniform success."

Tait continued to use this method and was quite satisfied with the results. The subject was later on taken up by Skene, who published a work on " Electro-Hæmostasis in Operative Surgery " (1901). He gives the same reason as Tait does for trying to get rid of the ligature, but although he mentions Keith's work with the cautery, he says nothing about Tait's work. Skene did not invent his instruments, they were the work of Louis Pignolet.

We used Skene's electric forceps for a number of years, and found them most satisfactory, and we think that for a septic appendix, or a septic pedicle of any description, this method is most excellent. Our objection to Skene's book is that he neglected to mention Tait's pioneer work ; this he should have done.

Later on, in 1896, we find him writing a short letter to the *Lancet* on the possibility of Ovarian Pregnancy, and one on " An Acoustic Bladder Sound and Vaginal Stethoscope " ; but these letters are of no importance.

In the *Lancet* (February 6, 1897) Tait writes on " The Treatment of Myoma," and objects to a statement that had been made in which it was said that the Gynæcological Society had for three or four years insisted that the extirpation of the uterus was the best treatment for myoma of the uterus. He said that he thought that such an operation was premature. The letter, however, shows that Tait himself had been practising the operation, for he says :

> I have had a number of cases of the so-called " panhysterectomy," and have a list much longer than any yet

> published in England, and I do not find that it commends itself to me in preference to Koeberlé's method, save in the one particular of being somewhat easier. . . . Its primary mortality, as far as my own work is concerned, is much the same as the clamp operation.

To the May (1) *Lancet* Tait sends a notice on " A new pattern of Scissors for Perineal Repair." He says: " The joint is a beautiful French contrivance which I first saw on a pair of catch-forceps, and have adapted it to scissors, where it has the important advantage of keeping the cutting edges of the two blades in accurate adaptation."

This scissors is generally known as " Tait's perineal scissors," the lower blade being the one with the sharp point. As we have pointed out in another part of this work, scissors made on the same plan were used years before Tait's time in operating on cleft-palate, and we have a pair at least sixty years old in our possession, and which we employ when performing Tait's " H " perineal operation.

In the *Lancet* (April 24, 1897) there appears under the heading " The Question of the Second Ovary in Ovariotomy ? " a letter from Clement Lucas, one of the surgeons of Guy's Hospital. He writes as follows in his opening sentences:

> The operation of ovariotomy requiring little anatomical knowledge, I have always inclined to rank as one for which little surgical training was requisite. Its former reputation depended entirely on the terrible mortality that in years gone by attended the earlier operations, so that a certain amount of courage was necessary in order to undertake it. Great credit was therefore due to those who, by greater attention to aseptic methods and detail, brought the operation to the present degree of safety. Whilst from a surgical point of view a little coolness and courage, with a very limited knowledge of the human body, are all that are requisite for an ordinary case, it is nevertheless an operation which requires a large amount of common sense in dealing with adhesions and with complications that may be met with.

Lucas then considers the " rule of the profession in dealing with the second ovary." To this letter William Duncan, Alban Doran, and Tait replied. We give the letters of

Duncan and Tait to show how they regarded the matter. Duncan says :

> The points raised by Mr. Clement Lucas in his letter of April 24 are both interesting and important. I think most men will agree with me that the right treatment was adopted in removing a slightly cystic second ovary in a woman, forty years of age. I go further, and say that in *all* cases of ovariotomy near or after the menopause the second ovary should invariably be removed, whether it looks healthy, cystic or atrophied. With regard to the case of a woman, aged twenty-five years, whose second ovary contained a cyst the size of an orange, it seems to me that instead of leaving it alone it would have been better either to have removed it altogether or to have excised the cyst and left the ovary—a plan I have successfully adopted in several similar cases.
>
> I am, etc.,
> WILLIAM DUNCAN.

Tait's letter is as follows :

> The extraordinary sentences which open Mr. Clement Lucas' letter made me look carefully at the top of the page to see that dates were right to make sure I was not reading a copy of your ancestor's journal of 1837. Is it possible that such a view can be held about any operation in surgery, much less concerning one about which there is always so much doubt till it is over ? Yet there the words stand. A woman with both ovaries removed for cystic disease does not become an " unnatural monster," nor is she cut off from the next or any other generation ; nor does she stand in the least danger from this cause of being divorced.
>
> Mr. Lucas is a lecturer on anatomy and surgeon to Guy's Hospital, and an examiner in anatomy at the Royal College of Surgeons of England, and he records this in your columns this morning : " She was only twenty-five years of age and was found to have a large unilocular cyst of her right ovary, which contained ten pints of fluid, and connected with the left ovary was a cyst as large as an orange. The right ovary was easily removed and the other was left." The tumour which he removed was not a tumour of the ovary at all, but a tumour in the broad ligament from which the ovary could easily have been separated by a skilled anatomist and left. The second tumour was probably the same, and if so, simple enucleation would have spared the patient the risk of a second operation and both ovaries. There is really no need to remove healthy ovaries at all ; besides, it is getting dangerous and had better be carefully avoided. Deliberately to leave cysts is, however, very bad practice, according to my experience and belief.
>
> I am, Sirs, yours truly,
> LAWSON TAIT.

In the *Lancet* (May 29, 1897) Tait has a letter on "Excision of Parovarian Cysts without removal of the Ovary or Tube."

The concluding paragraph of this letter is of interest for he says: "The conclusion from an immense experience going over thirty years is that such a thing as a unilocular tumour of the ovary does not exist save in one bottle which I placed in the museum of the Royal College of Surgeons as an example of an exception which proves the rule."

1897 was the Jubilee year of Queen Victoria. The *Lancet* had special aticles on the progress of medicine and surgery. It is interesting to notice what the writer has to say on the subject of the progress of gynæcology, and also to notice that Tait's name and work are not mentioned.

> To sum up, as regards gynæcological progress during the last sixty years in the direction of treatment, the most prominent achievements have been the establishment of ovariotomy and of vaginal hysterectomy, omitting minor operations, while in the way of diagnosis, the improvement of the speculum, the invention of the uterine sound, the introduction of means for dilation of the cervix uteri in order to examine the endometrium, and the bimanual method for examining the condition of the pelvic organs, stand out as the chief gains to be recorded. The accoucheur of to-day can hardly imagine what his practice would be without anæsthetics and without antiseptics, and similarly the gynæcologist finds a difficulty in realising what his art must have been without these, and with practically no way of investigating his cases except by digital examination of the vagina. Among the instruments that it would seem difficult for any modern operator to dispense with, we think Spencer Wells' artery forceps should certainly be named.

Spencer Wells probably inspired this article.

To the *Birmingham Medical Review* (May, 1897) Tait contributed a paper of great interest: "Was Robert Houston, of Glasgow, the first ovariotomist?"

This paper deals with a kind of ovarian tumour that few of us are familiar with, and we therefore give the paper in full; we have already given extracts from his papers in a former chapter.

(Reprinted from the *Birmingham Medical Review*, May, 1897).

Was Robert Houston, of Glasgow, the First Ovariotomist?

By Lawson Tait, M.D., L.R.C.P., Ed., etc.

Dr. Greig Smith has favoured me with a reprint copy of an article by him in the *Practitioner* for March, in which he answers this question in the negative.

For my own part, I regard all such matters as mere dialectic curiosities; and so far as concerns any possible claim of such a kind made for myself, I should not care one pin what the answer might be. Others, however, attach importance to them, and the grand-daughter of Ephraim McDowell has gone so far as to class me in the list of those who have "attacked" her illustrious ancestor, because I have indicated a slight interest in the history of Robert Houston, and have credited him with at least some importance in the history of this operation. Dr. Greig Smith goes almost as far, and has spent much literary skill and great industry in the preparation of a "library" paper. The misfortune of the paper is the absence of clinical acumen and logical accuracy, and it is this striking contrast in its constructive elements which induces me to reply to it, and to show, as I am sure I can do, that Robert Houston's case (1701) was an ovariotomy in every sense of the term, and that (be it of interest and importance or not) it is the first case of that operation of which we have any record.

In all such efforts the demands of logic must be supplied by definitions agreed upon, and here, of course, Dr. Greig Smith and I may differ. If we should do so our readers must judge between us. My first question for a definition, then, is "What is an ovariotomy?" and I think the answer is quite easy: "A surgical operation by which it is sought to cure a tumour of an ovary." Of course one ovary here includes two.

It will not do to say merely that it is a cutting operation, nor an abdominal section, because my old friend and colleague, the late Mr. Samuel Berry, did one ovariotomy in his life. He removed an ovarian tumour through a rent in the vagina made in the course of labour, tied the pedicle, and cut off the tumour—a complete ovariotomy.

It will not do to say that the operation must be "intentional," for many ovariotomies completed were not begun with any intention of doing ovariotomy at all.

It will not do, as Dr. Greig Smith seems inclined to believe, that the whole of the tumour must be removed, for in many operations large areas of the cyst walls have to be left, yet such may be perfectly successful, and are, so far as necessity goes, complete.

It will not do to say a pedicle must be seen and tied, for in many of the cases where transplantation by twisting and division of the pedicle has occurred completely, the tumour is removed by enucleation, and the pedicle is never seen. In some, indeed, the tumours have been found entirely loose in the abdomen.

It will not do to interlard, as Dr. Greig Smith does, the discussion with views on pathology; as to whether or not, for instance, when he quotes "showing that Houston believed with most of his compeers that 'ovarian dropsy' was simply a dropsy of the ovary," for singularly enough that is my belief to-day. Moreover, I do not follow, as I cannot understand, what Dr. Greig Smith says further, "and not a tumour at all, and that he would as soon have suggested amputation for dropsy of the leg as excision for dropsy of the ovary." As I cannot understand this, I may be forgiven if I say it is nonsense and quite beside the issue. We know very little of pathology, our advances alleged are generally confined to a change of nomenclature, with a very occasional and generally very unimportant extension of knowledge of minute structure.

Let us see how completely Dr. Greig Smith himself disregards this difficulty, and how completely I agree with him. He says: "Now in considering Houston's case we must be certain of the meaning of the words employed. The case undoubtedly records an operation of ovariotomy in the literal sense of the term, incision of an ovary or ovarian growth." Here he gives me my whole case, as I shall speedily show, only there is surely a misprint (or a very serious blunder in syntax on the part of Dr. Greig Smith). He should have said an incision for, not of. With this correction we are at one, for Houston is guilty of no such misstatement. It is clear he diagnosed an ovarian dropsy, and he opened the abdomen to cure it. He did not know what was going to happen as he went on—only the supernaturally wise amongst us know any better even now.

But Dr. Greig Smith goes on, and this is the chief element of error in his argument. It is claimed for Houston that he did ovariotomy in the usual sense, but, says Dr. Smith, "for this last operation two things are essential: one, that the tumour should be delivered from the abdomen; and the other, that its connection with the body should be severed. It is unnecessary to go beyond these two essentials; and there is not even a suggestion that either was carried out."

But they are not essentials, for it is not always either possible or necessary to deliver the tumour from the abdomen, as I have already shown; and in some cases there is no connection at all with the body, as I have also shown. Further,

the very details given so accurately and effectively by Houston show that his was a case where Dr. Greig Smith's "essentials" were quite unnecessary and wholly impracticable. In fact, they are not essentials at all in a very well-marked group of cases, of which Houston's was one, the first on record. Therefore, it is that there is not a suggestion in Houston's record that either was carried out. Therefore, it is that I made a mistake when in 1883 I said: "He must have seen and divided the pedicle." I was then as much in the dark as Dr. Greig Smith still is, and, like him, I believed in the "essentials"; but it is just because Houston so accurately describes what he did do, and says nothing about what he did not do, that his story is absolutely deserving of credence, and why he deserves the credit, be it worth what it may, of being the first to do an intentional, complete and successful ovariotomy.

Looking back on my thousands of ovariotomies I see now that there are many distinct groups to be made; each of these groups encloses numbers, great or small, which give characters different from the other groups, and these characters demand different views for their pathology and different methods for their treatment. Until our eyes are opened it is astonishing how blind we are. Spencer Wells and others, myself included, went on removing healthy ovaries along with simple broad ligament cysts until it became quite apparent that they only required enucleation and were thereby cleanly brought out from their extra-peritoneal bed without need, in most cases, of tying even one vessel. Removing the healthy ovary and tube was mere nonsense, and though no one persisted more strenuously against other people doing it than Sir Spencer Wells, yet he went on doing it. The group of dermoid tumours is another, far less common and having widely different conditions of pathology and treatment from any of the others.

Another group is one to which I venture to give, tentatively, the name of "digesting" tumours, for the phenomena of peritoneal digestion is certainly a leading feature in their history. There can be no doubt whatever of the accuracy of the extraordinary stories told of the disappearance of ovarian tumours. I have myself seen and diagnosed ovarian tumours and have watched them disappear, and now I have operated in quite a group of this special kind, and have caught a few of them in the actual process of disappearing. I am satisfied that they are all of one and the same kind, and that if we could only tell them in advance no operations would be necessary for most of them. But we cannot diagnose them, and they are too few in number to influence the general and best rule for ovarian tumours—remove them just as soon

as the operation can be arranged after they have been recognised.

This group is not a large one, for whilst I am quite sure I have seen a dozen I do not think I could dig out more than twenty in all, even if I gave myself up to the heavy labour of seeking them in my enormous volumes of records, and I am not disposed to take up the useless task. I remember some of them very clearly, and I have complete records of three of them available; and I have vivid recollections of some of the features of others, so that I can give a fairly accurate picture of their peculiarities, leaving it to others to confirm or correct, as may happen, when similar experiences arise to them. After reading what I have to say I think none of our younger ovariotomists will pass one of those cases without more careful investigation than any has yet received. The collection of the records of a few instances will prove of great interest.

Fortunately I have already to hand two independent records which fit into mine exactly, the first of which is Houston's own, and the second an example recorded in the most unprejudiced and interesting manner by my colleague, Mr. John William Taylor, to whom I am obliged for full notes of the case. He describes the tumour as a large multi-cystic tumour of the left ovary, contents semi-solid and gelatinous, which had already partially escaped into the peritoneum causing chronic peritonitis. So far the details are precisely those of the group to which I refer, though the "chronic peritonitis" as I have noted the appearances, are more those of the engorged velvet of a mucus surface engorged in the fulfilment of its function, such as the stomach presents during the process of digestion. The further details supplied by Mr. Taylor do not help me, particularly the detail of the pedicle which was treated by a wire clamp. From Mr. Taylor's first and less complete description I thought that the gelatinous contents went down to the base, but there it was more solid, and Mr. Taylor is of opinion that it would not have been possible to deal with it in any other way than the clamp, and I have no right to question his judgment.

As in other pathological groupings we have no hard-and-fast lines, so here we shall have complete and incomplete cases.

Fortunately from my own experience I can adduce a number of cases where the details are noted, or where they are very clear in my own recollection and in those of my assistants.

The features to be noted are that the walls of the cyst are peculiarly thin and are perforated at the thinnest points, the contents of the cyst being freely extravasated through the peritoneum. Usually there are more of the cyst contents in the cavity of the peritoneum than in the tumour. The

peritoneal surface, especially the visceral surfaces, are intensely congested, and are often covered with small flocculi, semi-transparent, but quite different from the purulent patches of lymph of chronic peritonitis, if carefully examined. My own belief is that the peritoneum is digesting and removing the exuded cyst contents, and therefore presents a physiological appearance in a form so exaggerated as to appear pathological. The exuding contents of the cyst are always gelatinous or viscous, a most peculiar kind of stuff not like anything I know elsewhere. Houston's description of it is very good. It will not flow through any trocar, and can be induced to get out of any sized wound only with the greatest difficulty. The hands are powerless to scoop it out, but it runs away rapidly with a stream of hot water, the only way of getting rid of it. As it dissolves in the warm water it becomes clear, as a rule, and shows clots as it flows in the stream just as soup jelly does.[1] When the abdomen is cleared, and the process is a long one, the cyst is found to be a movable bundle of shreds with vague pelvic attachment, whilst in the cleansing, pieces of cyst wall and secondary cyst masses are washed away. I have generally tied it in mass, cut it short, and dropped it back; but in some six or eight cases, so far as my memory serves me, there was nothing left to tie. This was notably the fact in the first case, of which I have clear recollection, a patient sent to me by Mr. Alfred Freer, of Stourbridge, and operated on by me many years ago. Then nothing required to be tied. In another, the attachment of the tumour was a soft villous mass on which two small bleeding points were secured, and the rest left undisturbed.

The contents in others vary considerably, but the main points are sufficiently constant to indicate close similarity, and with differences from the ordinary fluid-containing ovarian tumour. Of course in all these cases I used a drainage tube.

Mr. Christopher Martin, for five years my resident assistant, has favoured me with his recollections of two cases.

He writes: "I find I operated on Miss S. on February 29, 1892, you assisting me. There was a ruptured cystoma of the right side; the abdomen was full of apple-jelly fluid, yellow, and transparent; the tumour was almost sessile, the pedicle very friable; it tore through and had to be re-ligatured, and the abdomen was washed out and drained.

"I also remember the case at Swadlincote—that of a middle-aged woman on whom you operated on June 15, 1892. The abdomen was full of colloid stuff, which you washed out and drained. There was no pedicle to tie."

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[1] The only manufactured article I have ever seen to resemble this stuff is freshly made linseed tea. I never saw linseed tea till I had to consume a large quantity of it during the last ten days.—L. T.

I had forgotten this last case until reminded of it by Mr. Martin's letter. She was 57 years of age, a patient of Dr. J. Hay Moir, of Swadlincote. The tumour was of the left ovary, but most of its contents had escaped into the abdomen, and I washed only a mass of debris consisting of jelly and fragments of cysts. At the point of implantation in the pelvis was a mass of stringy fragments, which represented all there was for a pedicle, and nothing was tied.

The main features of the group are the singular cyst contents, the rupture of the cyst walls, the extrusion of the contents, and the consequent activity of the peritoneal serous surface; the destruction of the cyst and the evidence of its torn detritus in the cavity of the peritoneum; and, generally, an exceedingly vague area of attachment and marked absence of prominent blood vessels.

It was only after a comparative leisure came to my life by the discontinuance of hospital work, that I began to think it worth while to stitch together casual notes and recollections, and the re-reading of Houston's vivid description fixed this group in my mind at once.

I said in 1883 he must have tied the pedicle. But I know now that he did not tie the pedicle, because he does not say so, and that the pedicle did not need tying. He had tumbled across one of these "digesting" cases. The tumour had ruptured, and the contents were being extruded into the peritoneal cavity. He opened the abdomen and was not a little surprised, after so large an aperture, to find only a glutinous substance stopped up this orifice; all the difficulty was to remove it; he tried his probe and fingers, but all in vain; it was so slippery that it eluded every touch and the strongest hold he could take of it. No words of mine could excel or improve this description of the extraordinary stuff. "He took a strong fir splinter, about the end of which he wrapped some loose lint, and thrust it into the wound, and by turning and winding it, extracted upwards of two yards in length of a substance thicker than any jelly, or rather like glue that is fresh made and hung out to dry; its breadth was upwards of ten inches. This was followed by nine quarts of such matter as is observed in steatomatous and atheromatous tumours, with several hydatids of various sizes containing a yellowish serum, the least of these bigger than an orange, which seemed to be part of the distended ovarium." Of course these hydatids were secondary cysts. He closed the wound in the method of his time, and (Dr. Greig Smith has not noticed) he drained the peritoneum. "He kept in the lower part of the wound a small tent, which discharged some serosities at every dressing for four or five days."

Let us now suppose that Margaret Millar, of 1701, had fallen into my hands in the year 1897, what difference would there have been in the operation? The anæsthetic would have enabled me to empty the abdomen in less time by a stream of warm water and to empty it more thoroughly, and I should have used water and glass drainage tube instead of a tent, and after these differences there is positively nothing! Probably my cleansing of these cases is wholly unnecessary, for after getting quit of the bulk of the gelatinous stuff and thereby reducing the work of the overladen peritoneum, that industrious membrane could nearly always finish the task satisfactorily as it did in Houston's case. Then we have gone clean daft about modern drainage. We have forgotten all about our father's tents and think drainage a new thing. It is nothing of the sort, so that Houston's operation was not only an ovariotomy, "a surgical operation by which it was sought to cure a tumour of an ovary," but it was a successful ovariotomy—the first of its kind, but a record success. It was done in a fashion almost up to the present date; in fact, if I leave out the doubtful washing, I could not have done it better myself, and with this little explanation I hope Dr. Greig Smith will remain satisfied.

The horror of the thing remains. For a hundred and ten years the surgical world was blind to the lesson, and it took us another seventy years to struggle into the light of this great fact, and to the conclusions of the two great innovators, Houston and McDowell. As a third horror, disregarding the splendid results of Nathan Smith (1821–24) with the short incision and short ligature, and Baker Brown (1858–66) with the complete intra-peritoneal treatment of the pedicle, we bungled on with Hutchinson's murderous clamp till 1878!

To the (September) number of the *Lancet*, Tait contributes a short letter, "The Late Dr. Braxton Hicks." This letter is dated from "Llandudno," the Welsh town to which about this time he removed. In the letter he says:

A very important omission occurs in your notice of the late Dr Braxton Hicks in the matter of his discovery of the rhythmical movements of the uterus in pregnancy. I am writing away from all books of reference, but I think I am correct in saying that he discovered and established the point completely in his own work. The discovery lay for years neglected till I pointed out that it is by far the most trustworthy of all the signs of pregnancy. Even now it does not receive the prominence from writers of textbooks which its value deserves.

We may remark here that clinical observation has shown that from the first weeks, at intervals of five or ten minutes, painless contractions do occur in the pregnant uterus, and, in the early months of pregnancy, these contractions can be appreciated by a bimanual examination. The sign, however, is *not infallible* as it has been observed in hæmatometra, and in some cases of soft myomata.

During the second half of the year 1897, Tait contributed very little to the *British Medical Journal*. To the October (30) number, he sent a letter on "Tait's Flap-Splitting Operation in the Perineum," the main object of which was to call attention to a small work on his "Perineal Operations"[1] that we had published, and for which he had written a preface and notes.

At the December meeting of the Birmingham and Midland Counties Branch of the British Medical Association, he contributes three short papers. The first deals with a case where he had removed the appendages, in 1889, in treating a multinodular myoma. The patient, for five years after the operation, had enjoyed perfect health, never having menstruated since the operation. She then became troubled with diarrhœa and straining, and when Tait examined her he found a mass attached to the uterus, extremely hard and fixed in Douglas' pouch. Tait operated and removed the growth which proved to be one of the nodules of the original myoma, and this had been calcified. His other cases were: a case in which he had removed the appendages for epilepsy, and a case in which the tubes had cheesy material which proved to be tubercular; this was a new observation.

In the *Lancet* (December, 1897) there is a short letter, "A Question of Title," and this letter tells us that Tait had now left The Crescent, where he had practised for so many years, and that his consulting rooms were now in Newhall Street.

Tait had gone to live in Wales, and he came to Birmingham twice a week to see patients; for this reason he no longer

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[1] Stewart McKay, "Lawson Tait's Perineal Operations and an Essay on Curettage," London, 1897.

required his house in The Crescent, so he had taken rooms at 195, Newhall Street, where he saw patients on Mondays and Thursdays at 2 p.m.

To the *British Medical Journal* (February 5, 1898), he contributes a note on Nephrolithotomy, which shows us that he operated in London as well as Birmingham, for he tells us that he had removed a calculus in London from a patient sent to him from Barbadoes. "The remarkable feature of the case was the nature of the calculus, accompanied by an extraordinary loss of pus and blood, and yet not the least invasion of the kidney structure by suppuration."

To the same journal (April 16), he writes a letter on "The Etiology of Typhus Fever." This letter brings out the fact that he knew Charles Darwin for he says: "Talking on the subject one Sunday afternoon with Mr. Darwin. . . ."

To the September (10) number of the *British Medical Journal* (1898), Tait sends a letter on "A New and Original Method of Making Casts." He says:

> I am sorry to have to disabuse the mind of Professor Peters that the process of making casts by means of paraffin is either new or original. I invented the process at Bathgate, where paraffin was plentiful, in or about the year 1864, and published it either in the *Medical Times and Gazette* or in *Nature* (perhaps both) about 1867–68. I have not Neal's invaluable *Digest* by me, or I could give the exact references. I did all that Professor Peters has done, and more; I took life-sized casts of the living head and bust.
>
> I did all this with paraffin, a rough painter's brush, a sponge is not necessary, and it certainly must be expensive and cumbersome; besides, it cannot be carried about. A far better way of setting the mould, than by laying in threads, is my plan of using a fine saw of watch-spring steel. About the same time I advertised paraffin as a substance for the "appareil immobile," superior to starch or plaster, and though similarly recommended many years after by Professor William MacEwen it never seemed to have obtained the popularity it certainly deserves.

Tait still continued to attend the meetings of the Birmingham and Midland Counties Branch of the British Medical Association, for at the October meeting (October 13, 1898), he showed a cyst of the parovarium which he had removed from a patient two days before. He exhibited the specimen

because he was forced to remove the Fallopian tube with the cyst and leave the ovary behind: "The ovary had been preserved without the tube, a useless gland without its duct, and the principle of conservative surgery, as here applied to the uterine appendages, had been defeated." Tait did not take into account the particular secretion of the ovary that has its influence on the body.

At a meeting of the same branch (December 8), he showed an instrument which he had repeatedly used with success in breaking up calculi found in the gall-ducts. One sentence shows us that Tait had not followed one of the advances in gall-duct surgery, for he says: "The success met by Mr. Barling and others in freely laying open the ducts and removing the stones entire was alike gratifying and surprising, but, for his own part he would feel safer in leaving the ducts intact."

To the *Birmingham Medical Review*, May 1899, Tait contributed a paper on "Bedsteads."

1898

To the *Lancet* (May 21, 1898) Tait sent a short letter, saying that McKenzie Edwards, of Edinburgh, one of his old teachers, had taught him how to make the incision in the mid-line of the abdomen and to turn the rectus aside, and that he had followed this plan for thirty-five years.

To the *Lancet* (June 25, 1898) he contributes a "note" on "Lumbar Colotomy": the note taking up three columns. In this paper he says that he has always practised lumbar colotomy, and "personally I never gave any countenance to inguinal colotomy save in one case where I had abundant reason to be dissatisfied with it."

Tait contributed a short paper to the *Lancet* (February 11, 1899), on "The Treatment of 'Unavoidable Hæmorrhage' by Removal of the Uterus." In this paper he relates how, when called to a case in a collapsed condition for hæmorrhage due to "placenta previa," he at once proposed to do a Tait-Porro operation, which he performed with the aid of the

elastic ligature. The patient made a good recovery; the child living a month. He says:

> This case forms, as far as I know, a new departure. Whether it will receive a universal commendation is not the least a matter of doubt. The "mutilationists" will raise their voices against my proposals, but there are others who will follow my example, and to those who do this will surely come the comfort that they will diminish "the anxiety to the practitioner and the real danger to the patient" of the obstetric difficulty, which Simpson so graphically tells us, has more of both than any other.

This suggested treatment by Tait has been taken up with considerable confidence by Dudley, Krönig, and Sellheim, and it is now admitted that in a case where, as in a primipara, we encounter a very rigid cervix which we cannot hope to dilate, that Cæsarean section is the proper treatment. Whitridge Williams admits that he is in favour of the procedure in such a case, and Comyns Berkeley and Bonney also agree that it is the proper procedure to be adopted provided the patient can be operated on in a suitable environment.

In the January (21, 1899) number of the *British Medical Journal* is a paper by Furneaux Jordan, who was surgeon to the Birmingham and Midland Counties Hospital for Women. He says:

> Four or five years ago, under the influence of the teaching of Lawson Tait and C. J. Cullingworth, we might have looked upon this treatment of pus in the pelvis and pelvic organs as settled for ever. The brilliant results achieved by these operators, especially by the former, are only equalled by the clear and decisive way in which they have described the cases, with their pathology and treatment. The treatment laid down by Cullingworth in his inaugural address on Pelvic Abscess, delivered to the Midland Medical Society in the autumn of 1893, may be summed up as follows: For all cases of pyosalpinx, tubo-ovarian abscess, and ovarian abscess, removal in its entirety of the organ containing the pus through an incision in the abdominal wall. For all cases of extra-peritoneal suppuration—that is, of pelvic cellulitis gone on to suppuration—incision and drainage through the point in the abdominal wall above Poupart's ligament where the abscess usually points. Mr. Tait, in his book on abdominal surgery, advocates abdominal section and drainage for cases

of suppurating hæmatocele of the broad ligament, and even for cases of pus in the broad ligament.

In the *Obstetrical Society's Transactions*, 1892, vol. xxxiv. p. 254, is a most interesting paper by C. J. Cullingworth on the value of abdominal section in certain cases of pelvic peritonitis. Of 50 cases reported here in full there was suppuration in 30, chiefly tubal and tubo-ovarian; of these 30 cases 6 died, giving a mortality of 20 per cent.

In the last ten years of his work at the Women's Hospital, from 1883 to 1892 inclusive, Mr. Tait operated upon 71 cases of pyosalpinx with 11 deaths, a mortality of 15·6 per cent.

Tait replies to this communication in the following letter, one of the last that he wrote to any of the medical papers:

A Contribution to the Treatment of Pus in the Pelvis

Sir,—The paper on this subject by Mr. J. Furneaux Jordan in the *British Medical Journal* of January 21 contains so striking an example of the misuse of figures—it were gross flattery to call them statistics—that exception must be taken to it. He wishes us to accept the view that the death-rate of operations for "pyosalpinx" is 15 per cent. Mr. Furneaux Jordan has been my pupil, and it is therefore not unnatural that I should complain of his ignorance of my teaching. Either he ought not to refer to my writings at all—certainly he ought not to indulge in the flattering phrases he uses towards me, for under the circumstances they are merely jocular—or he ought to be familiar with my views and controvert them if he can. On the whole, I prefer to be left entirely alone rather than be misrepresented.

It may comfort Mr. Jordan to pick out a series of cases from my publications, and having made out a mortality of 15 per cent. for me and others, rest satisfied that we are, or at least have been, as bad as he is. But in the first place it is not true, and in the second the fact would be of no value if it were established. As far as argument for any purpose whatever Mr. Jordan might just as well pick out 100 cases of death in my practice and say that my mortality was 100 per cent.

In a volume published in 1889, I engaged in a laboured argument to show that cases of inflammatory disease of the uterine appendages for which operations have to be performed are of such kinds—mixed and varied—that it is quite impossible to divide them into classes on any accurate system, or with any advantage to our art. At p. 409, *et seq.*, I therefore publish a list of all the operations performed by me on such cases from January 3, 1880 up to May 6, 1889. They number

in all 474, with a total mortality of 2·5 per cent. The number is large enough for me to be able to say that the probability is that this represents not only my own mortality but the mortality of the operations in question, as they were the best results obtained up to that time, and I have yet to learn that they have been excelled. Mr. Jordan takes a different period, and I have not taken the trouble to go to the books and correct his figures, for his principles of selection are evidently peculiar, and they are not explained. But if I follow him in his own mistake I can pick out of my list 182 cases of pyosalpinx with 5 deaths; it gives a death-rate of 3·2 per cent. This must be very near the mortality of such cases, but even allowing for possible error—and it has been found difficult to print even the Bible without mistakes, and wholly impossible to reconcile its figures—and doubling the mortality it is a long way from the figures as twisted by Mr. Jordan.

But what rubbish the whole thing is will be seen by the slightest consideration of the variations in the cases given even briefly as they are in the proper column. These cases cannot be taken out in this way with any fairness, for I have seen a huge double pyosalpinx lying like a double parovarian disease, quite loose in the abdomen, and having no more risk to encounter in its removal than the parovarian tumours. On the other hand, I have recorded many cases which I enter as "chronic ovaritis" with adhesions so dense as to tax not only my utmost skill and determination to finish the operation, but my physical strength to overcome them. What nonsense it is to attempt to classify cases from a condition in itself of no importance, and very often not capable of determination until the operation is over.

I hope no one will follow Mr. Jordan's attempts at the use of figures until at least they have read what I have said in my book against it. Nor do I wish beginners to attempt to follow him in the "new method" of the vaginal route; and when they are no longer beginners they will not trouble about it, for the abdominal road will help them in all that should be done unless it is deemed advisable to remove the uterus as well, and then they will generally require to use both roads, finding it always safest to begin above, for then they will learn much that cannot be learned from below until it is too late.

1899.—Tait's Last Days

And now we have come to the closing scenes of Tait's life. He had continued to contribute papers to Medical Societies, and we find him during the first months of this year reading short reports on cases at the Birmingham Branch of the British Medical Association, and we have reproduced one of the last letters that he wrote to any medical paper when he replied to Furneaux Jordan's remarks on the mortality of operations on pus tubes. To the (June 10, 1899) *British Medical Journal* he contributed a bellicose letter on, "A Charge against the Chairman of Council" [Robert Saundby], and on June 13 he died. Immediately Saundby wrote to the *British Medical Journal:*

> I am sincerely sorry for the death of Mr. Lawson Tait. He and I were friends of long standing, and although we sometimes differed widely, the fund of geniality, which was the real basis of his character, never made these differences more than temporary. I feel sure that had death not intervened he would have recognised in time that I had made no attack upon him. During the last few years failing health had made him much more irritable than he used to be, and his old friends have marked the change with sorrow. Now that the grave has closed over his head I can only wish to recall the many great and noble qualities of the man we have lost.

During the last three years of his life Tait went to live at Llandudno. He came up weekly to Birmingham, to Newhall Street, in order to see and operate on patients, and he sometimes operated in London, or at other places.

His last contribution to the press was a note on "Bulb Culture," *Amateur Gardening*, vol. xv.

While at Llandudno he contemplated the erection of a sanatorium for consumptives, and with that idea in view he bought the "Old Telegraph," just on the summit of the Great Orme's Head.

Tait was present at the opening of the new railway station at Droitwich on June 3, and at the subsequent luncheon, in response to a call for his name when the toast of "The Visitors" was proposed, he rose and spoke the following words: "I came to Droitwich last night; I was quite well; I had a bath and to-day I am better!" Amid

laughter he resumed his seat with evidently little presage of what was so soon to come. On June 13 he was seized with pain, and serious symptoms connected with the kidneys, and died at three o'clock in the afternoon at his home at Llandudno. The real disease was of some duration, for two years before his death a calculus became impacted in his urethra. This was removed by a surgical colleague, but his health remained until his death indifferent, and no doubt the kidney disease was slowly progressing.

His remains were cremated at the Anfield Cemetery, Liverpool, and then placed in an urn, which was deposited in Gogarth's Cave, which was within his own garden, on the western side of the Great Orme's Head. This was done in accordance with his last wishes.

And now that he was dead what had his contemporaries to say about him?

The *British Medical Journal* immediately published a short notice in the June (17) issue.

> The announcement of the death of Mr. Lawson Tait on June 13, soon after completing his 54th year, will be received with very sincere regret. He has written his name large on the history of the great development of surgery which has taken place during the last quarter of a century. He was born and educated in Edinburgh, and settled in Birmingham twenty-eight years ago. The next twenty years were the fruitful time of his life. He brought to bear upon the practice of his art an acute intellect and great independence of mind, combined with boldness and manual dexterity. The influence which he exerted by his surgery was very great, and but for certain defects of temper and manner, which after all were chiefly on the surface, his position in the profession would have been much more influential than it actually was at the time of his death. He was a hard fighter, and was in the habit of using very direct and uncompromising language, but nothing can obscure the great services which he rendered to the practice of his art, and his fame and work will be remembered when the irritation, which his method of controversy was apt to produce, has been forgotten.

In the June (24) issue of the same journal is an obituary notice with a photograph. From this we quote one paragraph:

> Lawson Tait was no ordinary man. To see him once was to remember always his short burly figure, his leonine head,

his determined mouth and his masterful expression. Self-reliant, possessed of great ability, an indomitable will, a dauntless courage, and a consciousness of his own genius, he would have risen to eminence in any profession. He was an excellent companion, a good raconteur, a remarkably good conversationalist, and an admirable public speaker. He never attempted eloquence, but for straight plain business talk he was most effective, both in public addresses and in debate. No one could meet him without feeling the influence of his strong individuality, the vigour of his thought, and the originality and freshness of his views. He took part in many fierce controversies, both in the medical and the lay press. He was a born fighter, he revelled in the joy of conflict, and would fling himself with all the ardour of his pugnacious nature into the arena of debate. And when he hit he hit hard. Yet under this rugged exterior and brusqueness of manner, his friends recognised a large-hearted kindliness. He was passionately fond of animals, and was exceedingly kind to the poor and the afflicted ; scores of poor women were admitted into, and operated on, in his private hospital absolutely free of charge. By his death surgery has lost one of its great masters, whilst those who knew him intimately mourn a large-hearted and generous friend.

Enough, says a writer of the times, has been said to show that Lawson Tait will always be remembered as a born surgeon of unusual originality. His merits, widely recognised in his lifetime, will not be forgotten after his death, for he made a name for himself in the glorious history of British Surgery.

The *Lancet*, in its obituary notice, said :

Lawson Tait was appointed first Professor of Gynæcology at Mason University College, and afterwards bailiff and trustee of the institution, where he always espoused the cause of medical education with foresight and effect. In 1890 his surgical position was recognised by the British Medical Association, before which body he was chosen to deliver the address in Surgery at the annual meeting held in Birmingham. That address reads now as a perfect revelation of the man. In it we see his clear-headedness, his pugnacity, his contempt for detail, his worship of good surgery, and his glory in his own talents. To read it is to understand his enormous success as well as his more than occasional failures.

* * * * *

On his tombstone let us write the splendid words of Tennyson :

"Cold upon the dead volcano
Sleeps the gleam of dying day."

PART II

XXXVI

Personal Narrative of Tait's Method of Work

I now propose to write a sketch, which will give some idea of Mr. Tait's life at Birmingham when I first became acquainted with him in 1891. I shall describe, from the notes taken by me from day to day, his methods; then give some idea of his habits and private life; and, finally, briefly state some of Tait's views on various subjects, mostly gleaned from conversations during the week-ends which I spent with him.

When I went to London in 1891 I took a letter of introduction to Dr. Granville Bantock, a well-known gynæcologist, and when I told him that I meant to make a special study of gynæcology he advised me to take a clinical course at the Soho Hospital, and later on to endeavour to see some of Tait's work at Birmingham. Up to that time Tait's name was just known to me as other names are known to any student of medicine, and, as I had only received my degree in 1891, I did not pay any particular attention to what Bantock had advised about Tait, but entered as a clinical student at the out-patient department of the Soho Hospital for Women, Soho Square, London. At that hospital there were about one hundred and thirty out-patients a day to be seen, and consequently there was abundant clinical material: but the staff was composed of men who had no teaching ability, and their operative work was absolutely beneath contempt.

After attending the clinics and operations for about two months I felt that I had had enough of Soho, and was contemplating a move on when the following simple event changed everything for me:

Among the medical men attending the out-patients with me was an American, whose profound ignorance of everything relating to medicine and surgery was so apparent that I became friendly with him, chiefly in order to get some amusement by asking him his views on cases that we were called upon to examine and diagnose. His questions would cause the profoundest astonishment to every one. For instance, after examining a hæmatocele in the broad ligament, due to a ruptured ectopic pregnancy, he asked the surgeon the following question: "Sir, how do you know that there is blood in a hæmatocele?"

This young man disappeared one day in July, and I saw nothing of him for three days; on the fourth day he appeared at the clinic again, and I asked him if he had been ill.

"Oh no," he replied; "just had a run down to Birmingham to see Lawson Tait."

"How did you get on?" I asked with some curiosity.

"Didn't get on at all," he replied.

"How's that? Wasn't he there?"

"Yes, he was there all right, but he wouldn't let me inside the house."

"But why?" I asked, more curious than ever.

"Well, I got to his house and a chap in buttons opened the door, and I told him I wanted to see Lawson Tait operate. The footman said: 'Excuse me, sir, are you an American?' 'Yes, I sure am,' I said.

"'Well, sir, Mr. Lawson Tait will not let you in to see him operate. I advise you to write to him. My orders are, that no Yankee doctors are to be admitted'; and with that he closed the door in my face, so I just came back."

"Well, that's too bad," I said; "but I'll bet you I'll get in?"

"How are you going to do it?" he inquired.

"Never you mind; I'll bet you a *fiver* that I get in. Will you bet?"

"Yes," said my American friend; and we arranged that I was to visit Birmingham and try my luck.

This was about the middle of July, so I sat down to

think out how I could beard this lion in his den, and I hit on the simple plan of sending him a telegram on the morning that I started from London. The wire ran as follows: "Would like to see you at your rooms to-day at twelve o'clock." I reckoned that he would think I was a patient and would see me.

I started off and duly arrived at Birmingham, and was driven to The Crescent, a quiet street in the very centre of Birmingham, not far from the Picture Gallery and the Chamberlain memorial fountain. I stopped at No. 7, and a fair young man in a footman's uniform opened the door, and I asked if Dr. Tait was at home. He said, "Yes," so I said that I had an appointment with him for twelve o'clock. He showed me into a room filled with books, where I was left for at least an hour, during which time I looked through some of the volumes.

Then suddenly the door was thrown open; a short man with a huge head, long hair, and a broad chest, dressed in a velvet coat, entered the room. I looked up from the book that I was reading, and after glancing at him continued to read, for it never entered my mind for one moment that this was Tait. However, he did not leave me long in doubt, for he said in a voice that was anything but pleasant: "Well! do you want to see me?"

I was so utterly flabbergasted that I almost let the book drop, and all I could say was, "I want to see Dr. Lawson Tait."

"There's no *Dr.* Lawson Tait here; if you want to see *Mr.* Lawson Tait follow me."

I followed him into his consulting room, and sat down in an easy-chair. He did not seat himself; he stood by his desk and said: "Well! what do you want?"

I thought to myself I might as well be killed for a lamb as a sheep, so I looked up at him, and said:

"I have come over from Australia to learn gynæcology, and they tell me that you know most about it, so I thought I'd come and ask you to take me as your *assistant.*"

I sat in the chair and smiled at him, and watched his face; for it exhibited a look of utter surprise. Then he

burst out laughing. He laughed, he roared; he seemed to think it was a splendid joke, so I thought that I would join in, and we both laughed.

At last, he said: "Well, this is the best thing I have heard for many a day; I must tell Greig Smith, and Mayo Robson, they have both been here as assistants"; and he began to laugh again. Then, as Xenophon used to write, "A wonderful thing happened." Tait walked over to the chair where I was sitting, and putting his hand on my shoulder said, "I will take you as my assistant, just for the fun of the thing": and that is how I became acquainted with Lawson Tait.

As it was lunch-time I thought he would be sure to ask me to lunch, but he did not: as a matter of fact he never had lunch at The Crescent, as he breakfasted about ten, and did not have anything more to eat until about seven or eight in the evening. He, however, took me to his secretary, a young Jewish woman, who told me when to present myself, and I left The Crescent well pleased with my visit. I returned to London, and when I told my American friend what had happened he disappeared from the clinic, and forgot to pay up the *fiver* that he had lost.

I presented myself at The Crescent again on August 3, 1891, and at two o'clock I saw Tait do his first operation—a hysterectomy for myoma—and at once I realised that he was a master surgeon. I will now describe some of his methods; and by taking various operations will try and give as graphic an account as possible of his manner of working.

Most of his operations were done at his private hospital at No. 7, The Crescent. The building had originally been a terrace, and the various houses had been converted into one large building. I think at one time that the special public hospital for women had been housed somewhere here. Tait himself lived in part of the building, and saw his private patients there. It was certainly not the sort of place that one would now select for a private hospital, for it was located in a street little better than a back lane, and in the very midst of a manufacturing town which, as I have related above, had not long emerged from an absolutely

PLATE XXIX.

TAIT'S PRIVATE HOSPITAL AT 7, THE CRESCENT, BIRMINGHAM. HE RESIDED HERE.

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unhygienic condition through the wise measures introduced by Mr. Joseph Chamberlain.

Tait's nurses were trained by his matron, and were instructed to do things, not as they may have seen them done elsewhere, but after his own particular methods. Thus, no antiseptics whatever were ever used; all instruments were cleansed with boiling water, soap, washing soda and turpentine. No wounds were ever touched by any fluid but plain water; and the dressings were simply clean pieces of "Gamgee," as it came from the chemist's shop.

On the back of the door of each sick-room there were posted printed instructions for the benefit of the nurses, the patients, and their friends. Some of these rules warned the friends, in language that could not be mistaken, against bringing to the patients any article of diet whatever. Tait told me that one of the reasons that led him to put up this notice about food was, that after performing a section in a private house the patient's friends fed her, on the quiet, with biscuits and port wine; the result being that she vomited continuously for twelve days, and both recti muscles sloughed away!

Tait had no operation theatre; the operations were always done in the room where the patient was to remain; the anæsthetic was given to her while still in bed, and when unconscious the operation table and instruments were moved into the room. The room in which the operation took place had as little furniture in it as possible, and no curtain, or hangings of any kind. The floors were painted and were devoid of carpets, and were prepared like a yacht's deck, caulked, so that the operator could wash out the abdominal cavity with gallons of water, and if a gallon or more fell on the floor then the ceiling of the room below did not suffer from this dearly-loved method, which Tait used in any case where there was any suspicion of sepsis; and always in cases where there had been a rupture of a tubal pregnancy into the peritoneal cavity.

The table on which the operation was performed was a plain deal board, seven feet long and two wide, placed on two wooden trestles. On the undersurface of this board

two transverse bars were screwed, and these coming in contact with the top piece of the trestle gave the whole structure added stability. This table served all purposes, but it could not be placed in the " Trendelenburg position," and Tait therefore lost one of the greatest " helps " that pelvic surgery has received during the last thirty years. On the deal board was placed a piece of macintosh, and above this a warm blanket and a pillow at the head of the table. When the patient was placed on the table she was unconscious, for the table was not moved into the room until the patient was well under the influence of the anæsthetic, and as soon as the blankets had been arranged her wrists and legs were secured by a couple of wide bands. This was a common practice thirty years ago, and it was done because the surgeon feared that if the patient " came out " of the anæsthesia and strained, her intestines might protrude, Tait always stood on the right side of the patient, and just behind him was placed a small oblong table on which were arranged a series of small tin trays filled with hot water, and in these trays were placed his instruments. He never had an assistant to hand him an instrument, he always picked it up for himself and so saved time.

In one corner of the room there was always a table with emergency instruments ; and near at hand would stand a Paquelin Cautery and a bottle of solid perchloride of iron. There was generally a fire in the room, and by the fire was placed large cans of hot water, which were used for the sponges, or for " washing out."

During an operation Mr. Tait stood on the right side of the table, the assistant opposite, and a nurse stood on the right side of the assistant with a basin to receive dirty sponges.

On the right side of the anæsthetist stood two nurses, one who superintended, and attended to the hot water ; while the other looked after the sponges, and washed them as she received them soiled from the nurse on the left side of the table near the assistant-surgeon.

Two other details may be mentioned. A small table was always placed to the operator's left, and on this table stood two basins ; one contained clean hot water for his hands,

the other had hot water for his sponges, which just before each operation, he took from a linen bag which hung by the window of the room. He made a point of counting them in a very audible voice, so that every one could hear, and so there was never any difficulty about the number of sponges used : he generally counted out six marine sponges from a bag that always contained a dozen.

The preparation of the sponges was an elaborate and tiresome process. There was nothing Tait was so particular about as his sponges, and the matron of his hospital was intrusted with this special duty ; and to the cleansing of the soiled ones after the operation.

As regards the anæsthetic, Tait usually insisted on a mixture of three parts of chloroform and two of ether given from a Clover's inhaler *without a bag*.

Everything is now ready for the operation. The patient has been anæsthetised in her bed, the table has been moved into the room and the instruments and sponges have been placed in position. Mr. Tait has been summoned, and as soon as he enters the room he throws off his coat, tucks up the sleeves of his shirt, washes his hands with hot water, nail-brush, and soap, dries them on a towel—straight from the laundry—puts on a large apron made of macintosh, and takes up his position. His assistant does the same ; no antiseptics are used in the preparation of the hands of the operator or the skin of the patient.

The operator having selected a scalpel, whose blade was as a rule not more than half an inch in length, counts out his sponges from the linen bag hanging by the window, and throws them into the basin of hot water on the table by his left side. Taking one of these sponges from the basin he turns to the patient and gives her abdomen a few rubs ; then the abdominal incision is quickly made.

To enter the abdominal cavity Tait never cut through the *linea alba*, he always cut to the right or left of it. On reaching the peritoneum the extra-peritoneal tissues were taken up by two pairs of forceps, and handing one to his assistant and holding one himself he makes the cut that carries him into the cavity.

The peritoneal cavity being opened, the forceps are taken off, and the operator inserts two fingers of his left hand through the incision and tears the peritoneum until the tear reaches the whole length of the abdominal wound.[1]

Tait seldom had occasion to extend the length of the abdominal incision in dealing with diseased appendages.

After the operation was finished and the abdominal wound was closed with silk the patient was lifted from the operating table to her bed, and various lines of after-treatment were adopted.

In simple cases the chief feature of the after-treatment was "masterly inactivity"; that is, the patient was placed in a bed warmed by hot-water bags, covered by *three layers* of blue-blanket, and nothing was given her to drink for twenty-four hours. Opium was conspicuous by its absence. Tait was not a believer in drugs and often said when asked about his after-treatment that he had only three drugs in his hospital, *i.e.* Mag. Sulph., Seidlitz Powders, and Morphine, and with these three remedies he contented himself. An American surgeon, who had had an opportunity of seeing his after-treatment of section cases, once said: "Mr. Tait used three drugs, the chief of them is water, and the other two are the nearest approach to water as possible."

Tait used to give morphine in some cases, and he always ordered it when he knew that the patient would feel very great pain on regaining consciousness after a severe operation, or when there was great shock; and in such cases he often began the after-treatment by an injection of a pint of champagne into the rectum.

I will now go more into details as regards the various operations that he frequently performed.

Ovariotomy.—The patient was prepared the night before by a purge, and an enema was given early in the morning of the operation. The skin was cleaned with soap and hot

[1] The incision first made was never longer than two inches in length to begin with. Tait's idea of the small incision was that the state of abdominal surgery left much to be desired in accurate diagnosis, consequently many incisions had to be regarded as merely exploratory ones; once having satisfied himself as to the exact conditions to be dealt with, the opening was enlarged if it was found necessary to do so.

water, and in some cases turpentine was also used, before the patient was placed on the table; but the pubic hairs were not shaved. The bladder was emptied, and he took great exception to Greig Smith when he wrote that this was not necessary.

The incision having been made, and the tumour exposed, a small incision was made in its wall and the operator pushed his blunt *ascites tube* into the fluid, and drained the contents into a vessel below the table by means of a rubber tube affixed to the end of the tube.

This *ascites tube*, as he termed it, was a plain bent tube made of metal. The point was blunt and solid, but there was an opening in the tube about one inch from the point. In former years Mr. Tait invented and used an elaborate trocar, something after the pattern of Spencer Wells' well-known complicated instrument: but as he grew older he insisted that all his instruments should be as simple as possible so that they could be boiled.

The *ascites tube* having been pushed into the tumour, as much fluid as possible was drained away, and the tube could then be used for breaking down the walls of any secondary cysts. When all the fluid had been drawn off, the cyst wall was clamped close to the trocar tube which was then used as a lever to help in withdrawing the collapsed tumour through the abdominal incision.

If there were adhesions, preventing the cyst from coming out of the abdominal incision, these were rapidly broken down; nothing in Tait's work struck me so much as the rapidity with which adhesions were quickly but gently broken down; and I have no doubt that this rapid rupture of the bands was the reason one saw so little bleeding in his practice. August Martin of Berlin, whose clinic I visited after leaving Birmingham, used the same masterly violence in dealing with adhesions; he took only eight minutes to finish a section for adhering pus-tubes. I have often seen him rupture the pus over the patient's intestines, and yet his results were splendid.

If during an operation in which many adhesions were torn through there was any inclination to oozing, Tait

generally relied on packing with very hot marine sponges, and if this did not suffice he would use the Paquelin cautery, and in some cases he rubbed a piece of solid perchloride of iron over the oozing surface.

The collapsed cyst having been pulled out of the abdominal cavity, the pedicle was then treated. A handled needle was taken and silk passed through the pedicle, and the Staffordshire knot was tied when possible. If the tumour was large, then a modification of this knot, which I have already described (see p. 138) as " Bantock's knot," was employed.

Tait always used a sharp pedicle needle, and in those days—a quarter of a century ago—it was a regular subject for disputation whether the blunt or the sharp pedicle needle should be used. It was urged that with a sharp needle you were liable to go through large veins, and so get oozing. This oozing if it were a drop a second would amount to a fair-sized hæmatocele after some hours. Tait used to say that he had several objections to this theory of the origin of post-operation hæmatoceles: his first objection was that the hæmatoceles that were not infrequent in those days did not usually appear till several days after the operation; that men who used blunt needles frequently got hæmatoceles; and lastly, men like Gusserow, of Berlin, used to put from three to six silk ligatures into each side of the broad ligament with an ordinary sharp needle in every operation that he performed on the uterine appendages, and yet he seldom had any post-operation hæmatoceles.

Looking back to this far-off period I think the explanation of the post-operation hæmatocele was, that the tumours that appeared a week or more after the operation were not always collections of blood, but were septic collections, and the sharp or blunt needle did not matter: it was the sepsis, or the want of asepsis, that made all the difference; to-day the post-operation hæmatocele has practically disappeared, and so the cause was most likely the septic silk ligature which opened the vessels by an ulcerative process.[1]

Having tied the pedicle, an inspection was made to see if

[1] Taylor in his article " Ovariotomy " in Clifford Albutt's System, 1906.

there was much oozing. If there was more than there should have been, then Tait often flushed with very warm water. He did this in two ways: sometimes he simply poured the hot water (not saline) into the abdominal cavity, and let it run out over the patient on to the floor. In other cases he ran the fluid out of a bucket by means of a rubber tube affixed to a spout at the side of the bucket, then, when the peritoneal cavity was full he lowered the bucket to the floor and the fluid was syphoned off. This latter plan he always employed in cases of ruptured tubal pregnancy, for it enabled him to suck the small blood clots out very readily. Tait seldom tried to get all the fluid out of the peritoneal cavity; he generally flushed until the fluid was not blood-stained, then he would insert a few large sponges to keep the intestines back, and introduce a glass-drainage tube through which the fluid found its way for hours after the operation was completed, either by its own action, or by the nurse employing a special sucking apparatus invented by the operator.

Tait may claim to be the surgeon who made "flushing of the peritoneal cavity" a common procedure; and it is probable that it will remain as one of the recognised procedures of abdominal surgery for all time.

The abdominal incision was closed in the following way: He took a straight needle with a long thread of silk, and beginning at the left side of the pubic end of the wound he went through the whole thickness of the abdominal wall, including the parietal peritoneum; then he carried his needle across to the opposite side and pushed it from within out through the whole thickness of the abdominal wall. He did not then cut the silk but continued to insert the needle in the same way along the whole length of the wound. Thus he had a continuous suture. He would next take the extremity of the silk as it emerged from the corresponding point on the right side, and *then cut the silk.* He thus had a free end to begin with again, and so he worked upwards till the wound was closed. The advantage of this was rapidity; also he needed only one piece of silk and one needle, and when there was only one continuous piece of silk he could separate

the loops and take out the sponges without fearing that the sutures would slip out of their places; as interrupted sutures will do unless their ends are seized with catch-forceps.

When the wound was closed it was smothered with powdered boracic acid; not, he would say, with any idea of antisepsis, but because after trying various applications to his closed incisions, he had come to the conclusion that boracic acid was the least harmful. Over this powder he placed a few small pads made of "Gamgee dressing," which was nothing more than clean gauze, and it was used as it came from the drug store; there was no process of sterilisation at his hospital. A few strips of plaister, and a binder, completed the dressing.

No further attention was paid to these simple incisions; they were left undisturbed for a week, then the dressings were removed, and a stitch here and there removed, if necessary, and a fresh dressing applied.

Tait had the most sublime faith in "washing out," and also in the glass drainage tube. If a glass drainage tube had been used the dressings were always so arranged that the nurse could suck out of the tube any fluid without disturbing the pads placed over the incision.

For emptying the drainage tube Tait had invented a very simple little sucker, which consisted of a glass tube to one side of which was joined a glass reservoir. On one end of the tube was a flexible rubber ball which when pressed acted as a sucker, while at the other end was a piece of rubber tubing, which was inserted into the glass drainage tube. To work this little apparatus you first of all compressed the rubber bulb, then you ran the rubber tube down the glass drainage tube and slowly relaxed the pressure on the rubber bulb; the result was that the fluid ran up and fell into the glass bulb, and you got rid of the fluid, so collected, by again squeezing the rubber ball, after drawing the rubber tube out of the glass drainage tube.

If in any of the cases there had been much hæmorrhage the nurse was instructed to empty the glass drainage tube every quarter of an hour until very little fluid came away.

Tait's object—and he laid great stress on his point—was to try and keep the serum of the blood clot from accumulating, for he maintained that if much serum collected it dissolved the blood clots at the end of the vessels, and this favoured oozing and fresh hæmorrhage.

When I visited various clinics on the continent I explained Tait's views on this subject, but nowhere did I find these ideas received with any faith; and I might here remark that although I visited all the most famous clinics in France and Germany in 1892, nowhere did I ever see a drainage tube used after a section case: Clark, one of Howard Kelly's assistants, finally gave the pelvic glass drainage tube its death-knock in 1897.

Tait said, "When in doubt drain"; doubt in those days was the prevailing mental attitude, so the drainage tube was in constant use.

Tait said that he used a drainage tube if he had washed out the peritoneal cavity because of the escape of ovarian-cyst contents. I think the washing out was correct; many an ovarian cyst has unsuspected malignant elements in it, and washing out the peritoneal cavity is a wise precaution, but the drainage tube does no good in such cases.

After hæmorrhage from extensive adhesions, or for ruptured ectopic cases, he always employed lavage; experience had told him that blood-stained fluid in the abdominal cavity is absorbed very slowly; and we know now that it may become a medium of infection in the pelvic cavity.

As the fluid poured in it did, in some cases, help to unfold a coil of small intestine, so he regarded it as a friend and a help in preventing intestinal obstruction.

In no cases did Tait insist on washing out more than in all examples of papillomata of the ovary. He maintained that 50 per cent. of such cases could be cured by this lavage, and much evidence has confirmed his results.[1] In one of my own cases, after opening the abdomen I encountered a huge mass

[1] For we know now that under papillomata he included a group of non-malignant tumours and the malignant solid and cystic papillo-adeno-carcinoma; this will explain Tait's good results.

of papilloma: I did not attempt to remove it, I merely washed out. I sent the woman home perhaps to die. She did not die, the husband died, and she married again and is alive now, and it is fifteen years since her abdomen was opened!

On one occasion Tait put a glass drainage tube into the pouch of Douglas where the rectum had been torn; for fourteen days fæces came through the tube, and the patient recovered. The same thing occurred to me when operating on a fibroid of the uterus in which the most extensive adhesions were encountered. After separating the rectum a marked fæcal odour was perceptible, and though no rupture of the bowel could be detected yet it was deemed expedient to insert a tube and for weeks fæces came through that tube and the woman recovered.

Tait always used a drainage tube in cases of an abscess of the broad ligament where the opening had been united to the edge of the abdominal wound. He used sometimes a Koeberlés glass drainage tube, and in other cases he used the wire tube introduced into surgical practice by Chassaignac in 1859. I don't ever remember seeing him use a rubber drainage tube; probably this was prejudice, because Lister had introduced this form of drainage into England, not knowing about Chassaignac's work; employing it for the first time after operating on Queen Victoria in 1871.

Tait always used a drainage tube if he had encountered a broad-ligament cyst and expected some oozing into the cavity left after the enucleation.

Fibroid Tumours of the Uterus.—Tait treated fibroids of the uterus in three ways: he removed the appendages in those cases where he encountered what he called a multi-nodular fibroid, while he performed extra-peritoneal hysterectomy for what he called the *soft œdematous fibroids*, that is for cases where degeneration had occurred in the original tumour. I seldom ever saw him do this intra-peritoneal operation for fibroids, although he has recorded the fact that he often did it between 1892 to 1899; but he did not like the operation, and no doubt found it difficult—although he denied this—because he refused the help of the Trendelenburg position, which makes the operation so easy.

From the year 1880 to 1889, Tait removed the appendages from 262 cases of myoma with only four fatal results.

With regard to his results from hysterectomy by the *serre-nœud* method, described in a former chapter, he had at first bad results: 57 operations with 20 deaths. As his experience grew larger his results improved, and he had series of 15, of 33, and of 44 consecutive cases without a death; this last series being, at that period, the best on record for this operation.

Diseased Appendages.—Few operations in the present, or in the past, can present more difficulties at times than those performed for the removal of a pus tube. When we encounter a case where the tubes are bound down by multiple adhesions, and where the intestines, the bladder, the rectum, and the ureters may be involved, the operation may become one in which the highest surgical skill is required, and nothing in abdominal surgery can test the judgment and skill of the surgeon more completely.

To see Tait operate on one of these complicated tubal cases was to see him at his best. Seldom did he have occasion to enlarge his two-inch incision, and many a time have I seen him operate when almost nothing of his technique could be seen, because he relied on his sense of touch to guide him, and not until the distended tubes had been ligatured and removed could one tell what the lesion was that he was dealing with.

When the tube was much distended with pus he would often take a long curved trocar and draw off the pus, and then, without withdrawing the cannula, he would use it to lever up the empty tube.

Tait was without doubt at a great disadvantage through not employing the Trendelenburg position, which would have enabled him to see what he was doing; but as his table was but a plain deal board it was impossible for him to employ the Trendelenburg position. The fact that he did all his manipulation by his sense of touch spoke volumes for his wonderful dexterity. August Martin's method of making a slash into the abdominal cavity and then pulling the pus tubes out with dramatic violence was a method that had

only to be seen to be avoided, and yet his success was wonderful, for in Berlin in 1892 I saw him do thirty-two of these cases with only one death.

I saw Tait operate on many cases of hydrosalpinx, and I often examined the specimens, and I came to the conclusion that one of the factors in the cause of this lesion was the fact that Müller's duct must at times remain closed and does not split into the Fallopian fimbriæ. Mr. Tait agreed with this conclusion, as he pointed out that this was possible, because in the embryo the ostium of the Fallopian tube is a new formation, and not the original terminal opening, the more anterior portion of each duct being perhaps represented by the hydatid of Morgagni.

When Tait had to deal with a pedicle that was too frail to take a silk ligature, he was wont to seize the tissue with a pair of long catch forceps and leave them protruding through the abdominal wound for forty-eight hours. After leaving Tait I often saw Péan adopt this plan as a routine in many of his operations. I have often had resource to it in vaginal hysterectomy, and in removing a kidney when it was impossible to tie off the pedicle.

Ectopic Gestation.—I had been a few weeks at Birmingham when a telegram arrived for Tait to go to Bournemouth. On reading the words of the telegram he remarked, "I think this must be a case of ruptured tubal pregnancy."

The bags of instruments were soon ready, and we left by the first train. Tait had ordered a reserved carriage, and when my friend Hawkins-Ambler and myself arrived at the station we found Tait in the reserved carriage, and as soon as the train started he said that he was hungry and produced a basket with cold game and some special wine. The man that had been so austere for the last few weeks was a different man on that journey, and delighted us with his conversation. Ever ready to impart knowledge he told us of his first cases of ectopic pregnancy, and was quite sure that we were going to one of these dramatic operations.

We arrived about six o'clock at Bournemouth, and proceeded at once to the patient's house. Tait examined her, and in a moment said that it was a ruptured tubal case and

that he would operate at once. On entering her room we saw that the patient was profoundly anæmic and almost pulseless. Tait refused to allow the medical man who had called him in to the operation to administer the anæsthetic, and one of us did that while the other helped at the operation. He opened the abdomen with his usual small incision, turned out twenty ounces of blood clot, tied off the ruptured tube, washed out the blood clot, and was not at all particular about the water pouring down on a beautiful carpet in the bedroom: the patient recovered.

A few days later I asked him why he did not use a tub under the table to catch the water used in irrigation. He replied laconically: "The husband is the biggest manufacturer of carpets in Great Britain."

This operation was performed nearly thirty years ago, and these operations were not as common then as they are to-day, and this one necessarily made a profound impression on me. The dim light, the large amount of blood clot washed out, the collapsed patient, the superb confidence of the operator, and, later on, the complete recovery of the patient. The operation has been done by thousands of surgeons since Tait did his first case, but we cannot admire too much the man who in those dark days of abdominal surgery first determined to open the abdomen and stop the bleeding point.

Six months after this operation I had a very different experience in Berlin. A patient was brought in to a celebrated gynæcologist's clinic, and the chief of the clinic examined her under chloroform. He said—so my notebook records—"Gentlemen, we have here a case of ruptured ectopic pregnancy; the rupture has taken place into the abdominal cavity, and as you see by the anæmia of the patient she has lost much blood. As to-morrow is my laparatomy day I shall operate on her then, if she is still alive." The morrow came, the patient was again brought in—but this time pulseless—her abdomen was opened, forty ounces of blood were turned out, a ruptured tube removed, and in an hour she was dead! Perhaps she would have been saved if the operation had been done twenty-four hours

before. Whenever Tait operated on a ruptured pregnancy he always washed out the blood and clots with gallons of water: and now after all these years we are all glad to follow his example. He particularly directed our attention to the clots that collected in the pouch of Douglas, and in the *right and left kidney pouches.*

If he had to operate on a case where the fœtus had developed for some months and had died, he said that he laid down no definite rules as to the removal of the placenta, for he held that every case was a law to itself, since in some cases it may be very easy and proper to remove the placenta, while in other cases this might be an impossible and dangerous procedure.

I saw him operate on two cases where the fœtus had developed in the abdominal cavity, and it convinced him that it had either ruptured from the tube into the peritoneal cavity, and then continued to develop, because its amniotic cover had remained intact: or, it had first ruptured into the broad ligament and then again ruptured in among the intestines, still retaining its amniotic sac.

He told me that if called to operate on an ectopic pregnancy at term, the first thing to do was to decide which side of the middle line the opening into the abdominal cavity should be made. The incision should be made two or three inches away from the middle line and towards that side in which the pregnancy had developed. The sac having been opened, the fœtus was removed, and the umbilical cord should then be divided close to its placental origin, and the placenta should be emptied as far as possible of blood. The interior of the sac should be carefully cleansed of all loose membrane, and then filled and washed thoroughly with warm water, and the sutures carefully placed in the wound so that when they are drawn tight the sac will be hermetically closed. He did not advise the removal of the placenta: he said he had tried all ways, and he was disposed to leave it and let it take its chance.

GALL-BLADDER.— Tait said that one of the greatest surprises he ever had received was when he opened a gall-bladder thinking that it was a *parovarian cyst!*

The woman was forty years of age and had a large abdominal tumour which presented all the appearance and physical signs of a parovarian cyst. He opened the abdomen in the middle line above the pubes and tapped the tumour, then he enlarged the incision and put his hand into the cyst. His hand went up towards the liver : not down to the broad ligament. When he had reached the end of the cyst he found a gall-stone which was as large as a filbert nut impacted in the neck of the gall-bladder. This he removed with some difficulty; he stitched the opening in the gall-bladder to the opening in the abdominal wall and inserted a drainage tube. No bile appeared until the morning of the third day, and the patient made a good recovery, and Tait saw her three years after and she was in good health. I asked Tait how much fluid there was in the gall-bladder, and he said eleven pints !

GALL-STONES.— Tait may have occasionally removed the gall-bladder, but, as I have said above, he was quite satisfied to open the gall-bladder, remove the stones and drain it, and if he found stones in the cystic or common ducts he broke them up with a needle, or worked them back to the gall-bladder, or down to the duodenum. He did not open the ducts by incision, in fact he took no part in the extension of gall-bladder surgery after he had founded it ; preferring to stick to his original plan of sewing the gall-bladder to the abdominal wound and draining it, as he considered this drainage was an essential part of the operation.

HYDATID CYSTS.— As regards hydatid tumours, after introducing the operation, not knowing of Lindermann's work, he continued to do the same operation, and never attempted the modification, suggested and practised with success by Knowsley Thornton, of removing the endocyst, fixing the ectocyst to the abdominal wound, and then closing without a drain. Thornton's method I have developed with much success in Australia.

KIDNEY.— Tait had had a very wide experience in the surgery of the kidney. In a paper published in 1889,[1] which was his contribution to a discussion on renal surgery

[1] *Brit. Med. Jour.*, November 16, 1889, p. 1085.

introduced by Henry Morris, of London, and David Newman, of Glasgow, he said that his first paper on the subject was published in 1884, but that his first operation was in 1874. In his paper he records 74 operations, 44 of which were for nephrotomy and 22 for nephrectomy. He does not describe his technique in this paper, but he remarks :

> Much has been written on the best method of reaching the kidney, whether for merely opening it or for its removal, and variously named lines of incision have been argued for by those who have had least opportunity of making use of them. The real fact is that the kidney is best reached by the most likely looking road, and, in this respect, different patients exhibit great variety of condition.

As a matter of fact Tait had a very peculiar technique when he performed nephrectomy, and in several operations that I saw him do he made an anterior incision, opened the peritoneum, reached the kidney, placed the wire of the *serre-nœud* that he used for his hysterectomy cases around the pedicle, and then removed the kidney, leaving the *serre-nœud* in the wound for several days. His mortality, in published cases, was 4 deaths in 22 cases, *i.e.* 18 per cent.

Speaking about results he said :

> That eighteen per cent. must be regarded as a low mortality for an operation as serious as the removal of an essential organ ; but when we look at a list of cases, 44 of nephrotomy with only 1 death, the record is little less than marvellous, seeing that it is not yet twenty years since I was plucked at an examination merely for suggesting that a suppurating kidney, discharging pints of pus into the bladder, should be made to discharge into the loin, or even be removed.

Appendicitis.— I never saw Tait operate on an appendix. He agreed that the appendix should be operated on, and had performed such operations when pus was present ; [1] but thirty years ago the surgery of the appendix was only in its infancy, and I never saw an appendix removed in any operation theatre in Europe in 1892, though I visited some twenty of the most celebrated clinics.

Minor Operations : Curettage.—Tait frequently per-

[1] *Brit. Med. Jour.*, October 5, 1889, p. 763.

formed curettage of the uterus, and usually dilated the cervix by means of his own dilators that I have described above. Of plastic operations on the cervix and vagina he never did any, but he frequently did his celebrated perineal operations. These operations are so difficult to understand that I have given an account of them at some length above, as I have seen them performed in all parts of the world, but never correctly. As I have pointed out, I think the simple **V** operation is of little use; but the operation for the repair of the complete rupture of the perineum—the **H** operation—is a very wonderful conception, and with the addition that I have made to it, *i.e.* dissecting out the ends of the sphincter ani and joining them together, I think it is the best of all the many procedures that have been suggested for restoring a perineum, when *complete* rupture has destroyed the action of the sphincter.

Exploratory Abdominal Incisions, and "Tait's Laws"

Tait was a warm advocate of "Exploratory incision." In his work on "Diseases of Ovaries," published in 1803, he says:

> For my own part, so fearless am I now of abdominal surgery, so splendid have been my results in fields of practice which, until three years ago, seemed hopelessly enclosed, that I venture to lay down a surgical law, that in every case of disease in the abdomen or pelvis, in which the health is destroyed or life threatened, and in which the condition is not evidently due to malignant disease, an exploration of the cavity should be made.

It must not be supposed that Tait was the first advocate of *exploratory* operations. Bryant, in an address delivered at the inaugural meeting of the Midland Medical Society at Birmingham in 1886, gave a very full account of the rise and use of explorations from his student days up to that period. When he came to deal with explorations of the abdomen he said:

> I must not allow myself to enter into the question of making exploratory abdominal incisions when any suspicion

exists of serious or suppurative troubles about the uterine appendages, and happily my so doing is not needed since you have in one of your members an advocate of the practice of extreme powers and of unquestionable success who would certainly go as far as, and probably further than, I can go in favour of exploratory measures in these classes of cases. Mr. Lawson Tait's experience, skill in operating, and success encourage him to do what men of less power and experience had better not attempt.

Tait tells us how he came to use exploratory incisions :

> However advanced a case may be, I never refuse to operate, for I have seen some of the most unpromising cases [of ovarian cysts] recover without interruption. Even when there is strong reason to believe that the tumour may be complicated with malignancy I make an exploratory incision to remove all doubt. In this way I find that my proportion of exploratory incisions is increasing, for whereas formerly I made an exploratory incision when I thought the tumour might be removed and when I proved mistaken, I now make an opening often when I believe the tumour cannot be removed, and here again to my great delight, I am occasionally in error. An exploratory opening never does any harm, and very often does a great deal of good.

After-treatment of Section Cases.—Tait lived at his private hospital at The Crescent, Birmingham, and he gave instructions to his nurses to summon him at any hour if the patient was not progressing favourably. He impressed on me over and over again that in order to do justice to your patient and to your operation frequent visits must be paid during the first three days to the bedside. No man could understand his subject if he failed to do this, and he said that frequently he had turned an almost hopeless operation into a successful one by prompt attention to the after-treatment.[1]

Tait had made many alterations in his after-treatment. When he first began to do section work he used to allow his patients to have during the first twenty-four hours ice, or iced water, and in some cases soda-water, and brandy, and

[1] Tait so impressed me on this subject that years after, when I had had abundant experience in abdominal sections, I wrote my work on the "Preparation and After-treatment of Section Cases."

champagne. Then nutriment was given during the second day in the form of chicken-broth, or beef-tea. In his later years he altered all this. In his earlier operations he had studied the strange thirst that invariably follows the opening of the peritoneum, even in a mere exploratory incision. At first he thought that this was a clear natural indication that the patient should have plenty of fluids, and plenty of ice. But he came in time to regard ice as one of the things which should be banished absolutely from the sick-room after a section, for it always increases thirst. In 1892 he wrote: [1]

> Gradually I found it wise to diminish the quantity of fluid after an operation to the vanishing point, and now I keep my patients for as nearly forty-eight hours as may be in absolute starvation—this being modified, of course, by age and previous exhaustion. I have, indeed, come to regard this thirst, not as an indication for the administration of fluids, but, on the contrary, as one indication that the traumatism of the peritoneum has altered its flow and ebb in some strange way, and that the less fluid there is about the sooner will the natural rectification of the process be effected.

In his first ovariotomies he had had the catheter passed every six hours for the first three or four days.

> The intention of the use of the catheter is to keep the pelvis as quiet as possible, and for this purpose it is also well to administer occasional small doses of opium to prevent any action of the bowels for seven or eight days, even though there be no pain to necessitate the opiate. In some cases, the bowels may remain without action for ten, twelve, or fourteen days after the operation without giving rise to any uneasiness.

This he wrote in 1877, but all that was changed later on, and he went to the other extreme and became the great advocate for early action of the bowels after all sections, and in 1892 he wrote:

> Years ago I was driven to the determination to discontinue absolutely the use of opium by the mouth after abdominal operations, for two reasons: First, its action on the intestines is most certainly to modify and even suspend vermicular movement: and secondly, it masks the real condition of the patient. One dose of morphine given under the skin

[1] *Brit. Med. Jour.*, November 12, 1892, p. 1049.

immediately after the operation, for the double purpose of relieving pain and modifying shock is all that my patients ever get, and the great bulk of them do not get that. . . . I have a belief that starvation and withholding of fluid prevents the mechanical stasis of the circulation in the intestinal coats, which appears to me to be the initial stage of the fatal process of peritonitis, and this preventive measure I endeavour to assist by stimulating the peristaltic movement. . . . I have tried a vast number of different kinds of enemata—some suggested by my own thought and others suggested by ingenious friends—but I have always gone back to soap and turpentine. It is the business of any nurse watching one of my abdominal sections to note every six hours a set of four conditions—the pulse, the temperature, the occurrence of distension, and the passage of flatus *per anum*. So soon as the latter is freely and naturally established our anxiety ends, though our watchfulness is unremitting. The want of such passage for twenty-four hours after an operation, especially if accompanied by the slightest suspicion of distension, is dealt with without fail by the nurse herself, on her own responsibility, by the administration of a turpentine enema, unless she had been specially warned not to give it for some special reason, such as a rent in the rectum. If the turpentine does not answer the nurse reports, and a mild saline purgative is ordered—generally a seidlitz powder—and this is repeated every four hours until it acts. If there be any especial anxiety, such as the occurrence of vomiting in the presence of a clamp, 5 grains of calomel are given instead of the seidlitz powder. If the distension increases we never rest till we have had the bowels moved, and then our anxiety is nearly always at an end and our efforts rewarded by recovery. But above all things there must be no time lost, and nobody who may be called into consultation by anxious friends must be permitted to write a prescription and try some favourite mixture. So soon as this is threatened I repudiate all responsibility, and retire from the case. There is nothing in all this that I have not published years ago, but I am bound to take some trouble to repeat it on a favourable opportunity such as this, because it has been grossly misrepresented and misunderstood. I have never said that the purgative treatment will cure peritonitis, for peritonitis, once it is completely established, is practically an incurable disease, and almost uniformly fatal. But when it is completely established I do not know. Of this I am certain, granting a large number of cases going on to peritonitis, after operations or from other cause, if you subject the patients (not the peritonitis) to the purgative treatment, the number who will go on to incurable peri-

tonitis will be absolutely fractional compared to what will be the result if they are left alone or submitted to any other treatment which has come under my notice. "The practical outcome of my empirical experience is this, that the purgative treatment of peritonitis or threatening peritonitis, or of anything which seems to be likely to turn out peritonitis, if it be promptly brought to bear on the case, will gain the all-important time, and often it gains such time as will call into action that influence which will eventually turn the scale in the favour of the patient."

With regard to *time*, Tait maintained, "time is the most important element in a case of peritonitis from another point of view. It is my opinion that the outcome of a case of peritonitis depends far less on the severity of the symptoms than on the time over which it runs."

If I were asked to pick out one thing that Tait thought essential in the practice of abdominal surgery I should at once say that he considered that a good motion twenty-four hours after an abdominal section was the most favourable event that could be reported. Thirty years of practice has not diminished the importance of this fact in my mind, and I have often said that the one anxiety that I have had during the last thirty years, during which time I have performed over three thousand sections, is, that on inquiring how the patient is I should receive the report that the nurse *has failed to get the bowels to work!*

This point about the early action of the bowels has engaged the attention of every surgeon who has practised abdominal surgery; and as the years have passed its significance has decreased, because we now look at the whole situation from an entirely different view point. Peritonitis to the surgeons of to-day is the result of contamination by germs. Tait would not admit this; he shut out what we know to be the chief etiological factor—the germ, and the products of the germ—and he sought to explain peritonitis by all sorts of ways. We can therefore say that he started from a wrong basis, but we cannot put aside his enormous *clinical experience* of four thousand section cases, and this experience in his day was without doubt the most far reaching of any living surgeon. Again, we cannot put

aside his wonderful *success*. We must therefore say that while Tait's ideas as to the *cause* of peritonitis were wrong, because he refused to recognise the absolute importance of the germ as the real agent, still he managed to pull his cases through. We are therefore led to the conclusion that in spite of the germ being the real cause of serious peritonitis after an abdominal section, Tait's methods of treatment were as a rule effectual.

Now it has always seemed to me that he viewed the fact that the bowels had acted in a suspected, or a real case of peritonitis, from a wrong point of view. He maintained that if he gave a purgative or an enema, and he got a good action of the bowels, he had little to fear. He thought that the purgative had been the real agent in bringing about the happy condition or result, and there were many able observers who backed him up in this opinion to a certain point. For instance, Treves was asked to deliver the *Lettsonian Lectures* in 1894, and he chose " Peritonitis " as his subject. He gave every consideration to Tait's purgative treatment, and he admitted that : " In a large proportion of examples of perityphlitis, and in pseudoileus which may follow after operation, the prompt evacuation of the bowels is often attended with the best results." He, however, firmly maintained that " aperients—in the ordinary sense —are of no avail in established general peritonitis. . . . In septic peritonitis—in the usual acceptation of that term—aperients are useless, and the same may be said of their employment in true perforative peritonitis." As regards Tait's views he said :

> Anyhow, it was pointed out by Mr. Tait and has been made evident to most surgeons who have followed his teaching, that if an action of the bowels can be obtained at the outset of the symptoms, whether by the administration of a purgative or by an enema, the trouble in a large proportion of the cases passes away and the patient makes a good recovery.

These words of Treves declare a fact which thousands of men who practise abdominal surgery know only too well, that if they administer an aperient and the bowels act,

the patient does well, but if they fail to get the bowels to move the patient often dies. Why? We cannot suppose for one moment that the bowel action makes all the difference between success and failure. We must regard the fact *that the bowels act* as a sign that the germs have not been able to create a poison sufficiently strong to prevent the peristaltic action of the bowels: the bowel action is to be taken as a sign that the poison has not overcome the patient. We may take it as proved that all peritonitis has its origin in germ infection, and that if we introduce the most minute quantity of certain germs the patient is doomed, and no treatment is of any avail. If we introduce doses of germs whose poison is of a less virulent nature the patient has a chance, and by getting the bowels to act we may help the patient to eliminate some of the poison.

When we look this subject square in the face we have to acknowledge that face to face with virulent peritonitis after a section, we are as helpless as we were fifty years ago. We purge, or we give morphia, and we do a hundred other things, and then the patient dies, just as she did in the past. We know we are impotent in such cases: the advances and the successes that we have made in recent times are due to *prophylaxis;* we have done little to reduce the mortality once general septic peritonitis is present. Drainage and Murphy's method of administering saline by the bowel are our chief helps, but these are of little use once the proper dose of the virulent germ is introduced into the peritoneal cavity.

XXXVII

Week-end Trips

It is now nearly thirty years ago since I enjoyed those week-end trips with Tait and his dear wife, and Hawkins-Ambler. All these friends are dead and gone, yet the memory of those happy days as we drove about the green lanes of England in August weather, when the beautiful words of Dante often came to my mind, " Nell' der dolce che dal sol s' allegra," [1] gives me great pleasure to recall. Sometimes Tait would be in almost exuberant spirits—particularly if the operation fee had been a very large one—and this exuberance was infectious, and each vied with the other to say something that would raise a laugh. Occasionally we tried to get a pun off, but Tait as a rule didn't care for puns ; but he forgave you if you could get one in with three or four syllables. One day when speaking of a comic opera called *Fatinitza*, produced many years ago, I told him that a young actress at supper, after the first performance in Sydney, had been asked if she could make a pun on the title of the opera. She thought for a moment, then pointing to the gravy-bowl, said : " Why, there's fat-in-it-sir." Tait admitted that it was the best pun that he had ever heard.

On another occasion we caught him with an atrocity. One day when we were driving near Knole, Tait was saying what a beautiful place it was, and one of us suggested that the proper words for its coat-of-arms would be " noli me tangere ! "

One Sunday evening Tait had a party of friends on his house-boat on the Avon. The evening meal was finished, and he asked me to bring the coffee up to the upper deck of the boat where his friends were seated. Just as I reached the deck the bell from the village church began to toll. Striking the side of the coffee-pot with a spoon, I said : " Listen ! "

[1] *Inferno*, Cant. vii, 122.

"The *coffee* tolls the knell of parting day." Tait laughed, but said that I had desecrated the most restful line in English verse.

Of the stories told on these expeditions I remember many, but have only room for a few.

Tait told us about a young man who had just started practice in a remote village. His first call was to the village blacksmith. He found him a huge man and very ill with pneumonia. After examining his chest he advised the man's wife to procure some leeches and apply them to the patient's side, and he took his departure. After some hours he returned to see how the case was progressing, and he heard much groaning as he approached the sick man's room. At the door he encountered the wife and asked her what was the matter. "Well, sir," said the woman, "you see you ordered leeches to bleed my good man, and I could get never a one, so I thought that I had better put the *ferrets* on him!"

I told him one about a schoolboy in a country school in Australia that he quite enjoyed:—

A lad came to school one morning and was late. As he entered the large single room, which often constitutes the school in many of the small up-country villages in Australia, the teacher made a dart for him and seizing him by the ear said:

"Tommy Jones, did you say that I was an old maid?"

"No, teacher, no, I didn't, I—I—I—sed your mother was!"

Another story, told by one of us, was a great success.

When Moody and Sankey, the evangelists, came to England, they created a great sensation and their meetings were attended by thousands. A life assurance agent, thinking that it would be a good stroke of business if he could get Moody or Sankey to insure in his office, approached Moody one day and said:

"Sir, will you allow me to insure your life?"

"Young man," replied Moody, in a solemn voice, "can you insure my soul?"

"No, sir, my office doesn't like *Fire* Risks!"

SPENCER WELLS.— Tait told us once the following story

about the man who seemed to be his *bête noire.* He said that he had been invited to attend a medical conference at Berlin. One night he was the guest of honour at a dinner given by August Martin, the celebrated gynæcologist. He had not been long seated when a note was handed to him, and on reading it he explained to Martin that he had promised to take an American surgeon's wife to the opera, and she was waiting for him. Martin at once said that the lady must come and have dinner with them. Tait left the room and returned with her, and she took a seat between Tait and Martin. As the dinner progressed Martin pretended to take offence at something Tait had said, and at once challenged him to a duel. Tait accepted the challenge, and said that as he was the challenged party he had the right to choose his weapon. "Certainly," said Martin. "What shall it be?" Tait replied: "The most deadly of all weapons, Spencer Wells' clamp!"

Tait was accustomed to give dinners at Birmingham to medical men who had called him to consultations, or who had sent him important cases for operation. I happened to attend one of these festive gatherings, and told the following story against my own countrymen.

Froude, the historian, paid a visit to Sydney many years ago, and is reported to have said to an Australian:—

"Well, you Australians ought to be fine men!"

"Why?" said the colonial.

"Well, you see," said Froude, "most of your fathers were sent out by the very best judges!"

Needless to say that the great historian would never have been guilty of such a gross insult; even though there may have been a grain of truth in the remark. English people like to say that their ancestors came over with William the Conqueror. Americans go back to the adventurous souls who arrived in the *Mayflower*, but Australians don't allude with any pride to any of their ancestors who happened to have come to Australia with Captain Cook!

Tait was rather fond of trying to puzzle out the answer to any riddle; he would sometimes take a week thinking out a suitable answer. One day, driving through Coventry, he of

course alluded to Lady Godiva and her story. Coventry was then famous for its bicycle factories, so after hearing Tait on Lady Godiva I asked him the following riddle :—

" If Lady Godiva were to come back and had to do her ride again, how do you think she would do it ? " He thought for a time and then gave it up, and I replied, " On a bicycle *without-a-tyre* (attire)."

One of the most laughable stories that Tait told us was about an old Birmingham chemist who was of such a kindly disposition that he prescribed for more people than any doctor in the town. One day in August, when the weather was very close, a countryman dressed in his best clothes, entered the shop, and going up to the counter said to the chemist :

" I want a dose of castor oil."

" Yes, sir," replied the chemist ; " pray take a seat."

The countryman sat down, and removing his hat mopped his hot forehead. Presently the chemist appeared through a glass swing-door and asked the countryman if he would like one of his cold summer drinks.

" My oath ! wouldn't I," said the man with a grin.

The chemist bowed and smiled and disappeared through the glass door, and presently appeared bearing a most inviting-looking pink beverage which he handed to the countryman, who swallowed it with great satisfaction. The chemist again disappeared through the glass door and the countryman sat on for some time, when, happening to look up he saw the hour by the clock on the shop wall. He jumped to his feet and approaching the counter said hurriedly to the chemist :

" Look here, old man, it's getting late, and I'm in a hurry ; what about that dose of oil ? " " Oh ! " said the chemist with his most benevolent smile, " you've *had* it ! " " I've *had* it ? " roared the countryman ; " Good God, man, it wasn't for *me !* I've got to get *married* in half an hour ! "

I once asked him a riddle which he said he would like to work out, but after spending some time on it he gave it up and was very annoyed when I told him the answer. He said that it was not a legitimate riddle, and was an impossible

one to answer: he, however, admitted that it was a good one.

This is the riddle :—

" Why is a sheet of foolscap paper like a lazy dog ? "

" A sheet of foolscap is an ink-lined-plane; an inclined plane is a slope-up ; and a slow pup is a lazy dog ! "

There was one joke that I got off on him that Mrs. Tait enjoyed. I think we were in Southampton, a town not very far distant from Tait's country house in the New Forest, and we were walking along past the shops when we saw a fat woman in a shop selling sausages. We all remarked how grossly fat she was. When we reached the hotel where we went to lunch, I disappeared, and did not come to the table until the others were seated. Tait asked me where I had been, so I told him that I had gone back to the shop to find out what the fat woman weighed.

" And what *do you think she weighs ?* " I asked him.

" Oh, twenty stone ! " he replied.

" No. Sausages ! "

Mrs. Tait laughed very much at his discomfiture, and for many a day after, whenever we saw a fat person we always said to him, " Now what do you think *she* weighs ? "

Once when we were on the Brighton road I told him a story I had heard in London from an old friend of Herbert Spencer.

It seems that the old chap lived with two old maids, and they took him out for little trips from time to time. Once they all went to Brighton, and the old maids pointing out the Brighton road said to Spencer : " Sir, this is the road along which George IV. used to drive when Prince of Wales."

" I take no interest in the criminal classes," replied Herbert.

It was with such light things that we contrived to pass away many a happy week-end : but these week-ends were invaluable to me, because when driving along a country lane, where " Nature dressed yet not disguised by art, wears its most alluring form," Tait did not object to talk shop, and if we brought up any surgical subject that we wished him to

explain he did so most willingly. He was a born teacher, and he had the gift of making a difficult subject very clear, for he had made a close study of the kindergarten method and used to say that if this method of teaching were more widely adopted in medical schools it would be to the advantage of the students. For instance, he thought that if you gave a student a bone to study after reading his text-book, the next step was to give the student a lump of clay and tell him to mould out as close an imitation of the bone as possible from memory, then to compare it with the bone and find out his errors, and take another lump of clay and remould it. This process fixed the bone and its surfaces more surely and accurately in the student's brain than mere reading from the text-book did.

One day, speaking of orators, he said that he considered John Bright the finest speaker he had ever listened to, and after him Annie Besant. He told us that before John Bright delivered a great speech he always shut himself in a room for some hours and walking about delivered the speech to the furniture. He said that he always had his large quaker hat near him, and when he arrived at the concluding sentences of his speech he would lift his hat and glance into it, for there he had the exact words written on a paper pinned to the lining.[1]

Two women he often spoke about: one was Harriet Martineau, and the other Florence Nightingale. About the former he said that her death excited considerable interest among gynæcologists, with the result that a *post-mortem* examination of her body was made known. All this curiosity was due to the fact that Miss Martineau, in the year 1855, had imagined that she was the subject of heart disease. Previous to this—according to her autobiography—she says as far back as 1839 she suffered from a tumour " of

[1] Since writing the above I have read Sir Edward Clarke's " The Story of my Life," and in describing his first speech in the House of Commons, he says: " The dull hours from eight to ten dragged along and then John Bright rose. Rowland Winn, the Whip, came to me doubtingly: ' Are you prepared to follow Bright ? ' It was the very chance I wanted; and while cheers were following his peroration, which was admirable in expression, but to my surprise, obviously read from his manuscript, I rose."

a kind which usually originates in mental suffering." She consulted the well-known gynæcologist of that time, Sir Charles Clark, who said that the tumour was an enlargement of the body of the uterus. From 1839 to 1844 she was a confirmed invalid. Finding that she got no relief from her medical attendants, she underwent a course of mesmerism, and in a few months was restored to health! Her recovery excited much discussion and controversy, and she herself felt no doubt either as to its remedy, or to its being due to mesmerism, and she published an account of her case in her "Letters on Mesmerism."

Some ten years later, she imagined that she was suffering from heart disease, and she passed the remaining twenty years of her life an invalid; but not as an idle one, for among her labours she contributed no less than 1600 articles to the *Daily News*.

At her death in 1877, Dr. King, who had been attending the patient for some years, got permission to hold a *post-mortem*, and he then discovered that Miss Martineau had a *large ovarian tumour!* He handed the specimen over to Spencer Wells, who reported on it to the Clinical Society of London. Wells said that the evident explanation of Miss Martineau's case was, that in the first instance, when she became confined to her bed in 1839, the ovarian tumour, mistaken by Clark for a uterine tumour, had been situated in the pelvis, and that during the next four years while the patient rested, the tumour had grown and had escaped from the pelvis, and that this escape had given her the relief that she imagined she had obtained from the mesmerism. When in 1855 she had consulted Sir Thomas Watson, she imagined that she had heart disease, although Watson told her that she had an abdominal tumour and not heart disease, but she thought that she was on the point of death, and remained the subsequent twenty-one years of her life a confirmed invalid.

English Inns.— As regards the English inns Tait said that the number in the old days could be accounted for because the roads of England had for centuries been so incredibly bad that a traveller was thought to have made

wonderful progress who got over twenty miles in a day by coach ; and as the dangers of the road increased a hundredfold when night came on it was imperative to have innumerable inns along every main route. Then it came about that the village inn, that has been extolled from the days of Chaucer to the days of Elizabeth, and on to the end of the coaching days, became such convivial meeting-places that the neighbouring gentry would leave their homes to pass the evening by the inn fireside, and enjoy the company of any stray travellers who were resting there for the night. As a consequence many of the inns of England became quite luxurious abodes, as Walton has told us, where the brick floor was swept clean, where the walls were stuck round with ballads, where the sheets smelt of lavender, and where a blazing fire, a cup of good ale, and a dish of trout fresh from the neighbouring brook, were to be procured at small charge. At the larger houses of entertainment were to be found beds heavy with silk, while choice cooking, and claret equal to the best that was drunk in London could be had. Old Dr. Johnson was so enthusiastic about the inns of his day that he declared a tavern chair to be the throne of human felicity.

Tait and Coffee Houses.— Speaking one day of the past Tait said that he would have liked to have lived in the days when the coffee houses were such important institutions. In those days when such things as public meetings and other methods of agitation had not come into fashion, and the modern newspaper was unknown, then the coffee houses became the centres of gossip whether the subject was new, or light, or grave. He said that he would have liked to have dropped into Wills', a house situated between Covent Garden and Bow Street, where the best things in polite letters were discussed every day ; and after leaving there we might have lingered at Garraway's, whither came every day John Radcliffe, who had the biggest medical practice in London. All important matters connected with medicine could be heard discussed in that room, which was really a social clinical room. Such delightful meetings would not be possible at the present day.

Tait had been largely concerned with the establishing of coffee houses and stalls in Birmingham.

England and the Bath.—Speaking one day about the evolution of the aseptic technique, he said that it was a fortunate thing for surgery that Syme was not only a man of good family and great refinement, but also scrupulously clean. He pointed out that social rank and cleanliness did not necessarily go together in the past, and if an examination of English castles and country houses was made, it would be found that not one of the old places had such a thing as a bathroom. This of course did not prove that the occupant did not have a bath occasionally; but away back in the middle ages, when many of these castles were built, bathing was absolutely unknown among any classes. He attributed this state of things to religious fanaticism; the heads of the Church taught that the Romans were the cleanest people and indulged to excess in the bath, at the same time they were the most immoral, consequently the Church made out that there was some connection between bathing and immorality, and forbade their flock to wash themselves to show that they had no sympathy with the immoral ever-bathing Romans. The result was that the people in the upper ranks of society became so odoriferous that perfumes were introduced in order that social intercourse might be made even possible.[1] These notions continued to remain in the mind of the English people, and bathing never became a popular pastime, so that the Englishman and his bath was a popular myth. This lack of fondness for the bath was combined with a brutality of disposition among the masses that is almost incredible until one takes the trouble to examine the details of the everyday life of the people, say of the seventeenth century, in England. Those who doubt these expressions of Tait should read Macaulay on Civilisation and its effects on the mass of the people. This writer says that things in Scotland were just as bad if not worse than in England. Let me quote

[1] Professor Pearce of the University of Pennsylvania confirms this: "Personal cleanliness was unknown; great officers of State, even dignitaries as high as the Archbishop of Canterbury, swarmed with vermin—such, it is related, was the condition of Thomas à Beckett, the antagonist of the English king."

one sentence from his "History": "A gentleman of Sky in Lochaber, whose clothes were begrimed with the accumulated filth of years, and whose hovel smelt worse than an English hogstye, would often do the honours of that hovel with a lofty courtesy worthy of the splendid circle of Versailles."

LISTON, FERGUSSON, SYME, SIMPSON, AND MARION SIMS.—Sitting round a peat fire in his home in the New Forest,[1] with his dear wife by his side (her name, I remember, was Sibyl, but he always called her Peter) Tait, after a good dinner—he loved a good dinner—and mellowed by some of his superb wines, was quite ready to tell you anything he could; and it was then that we got him to speak of the past.

Some nights he would tell you of men he had known; other nights he would tell you of his early struggles; sometimes, if you asked him, he would explain how he had evolved a particular operation that his name was associated with. There were certain things that we found that it was not discreet to introduce into the conversation; vivisection we never spoke about, and Lister's name we did not mention. If we wanted to "pull his leg" we asked him what he thought of Spencer Wells; and he started off like a horse under whose saddle you had put a Scotch thistle.

What I enjoyed most was to hear him speak of celebrated men that he had known, and he never tired of telling us about Sir William Fergusson, Syme, Simpson, and Marion Sims.

He knew many stories about Liston, and I have some notes of conversations which I wrote down either before retiring to bed, or when in bed next morning.

In speaking of brilliant surgeons he said that all hearsay evidence pointed to Liston as being the most brilliant operator that has ever lived; his dexterity was superhuman. The reason for this quickness was that Liston while he lived in Edinburgh had to perform his operations

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[1] An Australian poet, Will Ogilvie, has published (*Country Life*, Feb. 19, 1921) some verses on the "Ponies of the New Forest," and alludes to the country which lies about Tait's country house near Lyndhurst:

"Ponies up from the Forest, startled and swift and strong.
Fresh from the glades of Lyndhurst where the watchful roebuck lie;
Foaled in the blackthorn thickets that look to the Channel tides,
Wild as the hawks that hover over the Beaulieu rides."

before the day of anæsthetics, and this rapidity was evolved in order to spare the patient prolonged agony. But not only did Liston operate rapidly, but his work was superb. He had a gift and this gift was brought to perfection because he taught anatomy for years in Edinburgh, and he acquired dexterity from continually performing operations on the dead subject. Added to this he was a superb specimen of manhood. He stood six feet two in height, and was made in proportion: he also had magnificent large and strong hands.

One of Tait's stories of Liston was confirmed when I read years later William Cock's description [1] of the first major operation performed under ether anæsthesia in London. It was carried out by Liston at University College Hospital in 1846. The operation was an amputation through the thigh. The ether was administered by William Squire, from an inhaler devised by Peter Squire, a well-known chemist of the day.

Cock says that the operation theatre was full. The fact that the experiment was to be tried had become known, and surgeons, physicians, and students had come to see what would happen.

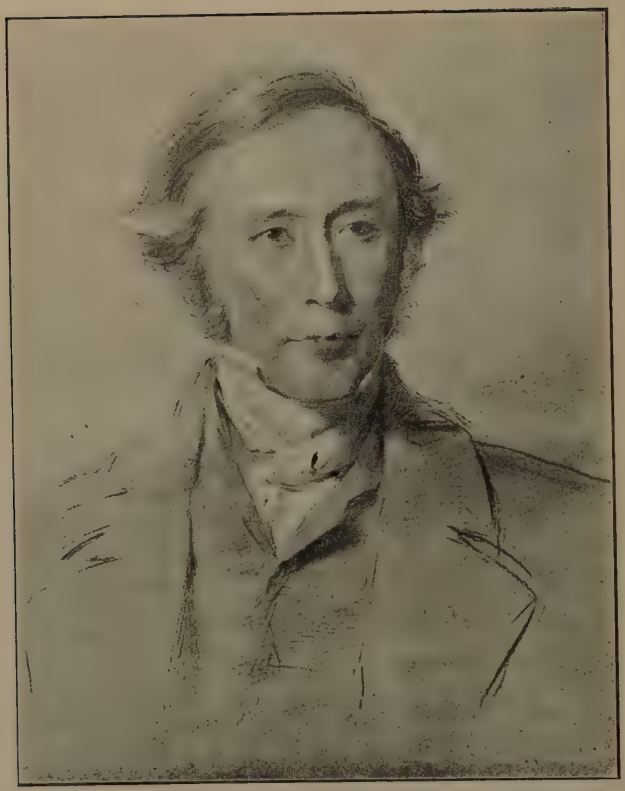
Liston entered the theatre—a splendid specimen, his splendid physique at once attracting the attention of all the strangers present—and walking towards the spectators, he calmly said: "Gentlemen, we are going to try a Yankee dodge to-day for making men insensible."

This is the memorable scene that Tait painted for us: the patient has not yet been brought into the theatre, so while William Squire is getting the primitive inhaler ready, Liston takes from a case one of the long amputating knives with which his name is associated and tries its edge, and when Mr. Ransome, his house-surgeon, has got the saw, two or three tenacula, and the artery forceps named after the operator, as well as the waxed hemp ligatures ready, Liston orders the patient to be brought in.

The patient having been carried in is placed on the table, Squire administers the ether, holding the patient's nostrils,

[1] *Amer. Jour. Surg.*, Anæsthesia supplement, July, 1915, p. 98.

PLATE XXX.



JAMES SYME AS A YOUNG MAN.

From a Drawing in the Scottish National Portrait Gallery.

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and Liston stands by trying his long knife against his thumb-nail. Squire looks up and says, " I think he'll do, sir." Liston takes his place, Cadge is told to take the artery, Ransome holds the limb. " Time me," says Liston to the students.

> The huge left hand grasps the thigh, a thrust of the long, straight knife, two or three rapid sawing movements, and the upper flap is made ; under go his fingers and the flap is held back ; another thrust, and the knife comes out in the angle of the upper flap ; two or three more lightning-like movements, and the lower flap is cut ; under goes the great thumb and holds it back also ; a touch or two of the point, and the dresser holding the saw by its end, yields it to the surgeon and takes the knife in return, half a dozen strokes and Ransome places the limb in the sawdust.

Twenty-five seconds ! The femoral artery is tied, five or six vessels are tied, a strap of wet lint is put between the flaps, and the operation is over. The patient wakes up and trying to raise himself says, " When are you going to begin ? Take me back ; I can't have it done." He is shown the elevated stump, he drops back and weeps a little and is removed to his ward. Five minutes have elapsed since he left it. As he goes out Liston turns to the audience so excited that he stammers, and then exclaims : " Gentlemen, this Yankee dodge beats mesmerism hollow."

Liston died in 1847 at the age of fifty-three from an aneurysm of the aorta ; consequently all that Tait had to tell us about him was mere hearsay. It was different when he came to talk of his greater master Syme.

In Syme's younger days he and Liston had been like David and Jonathan.[1] They were related in a distant way, and it was through Liston's influence that Syme took to medicine as a profession.

Unlike Simpson or Reid, he was born a gentleman, and was never in want of money, and it is to his credit that with the means to live as a country gentleman, he persevered in making the practice and teaching of surgery the business

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[1] Miles, " The Edinburgh School of Surgery before Lister," Black, 1918, p. 161.

of his life. His heart was in his work; he lived for surgery, and he lived for his pupils. In diagnosis he was as careful to anticipate every possibility of failure as was the Iron Duke to avoid risking a battle with the chance of defeat. As an operator his nerves and coolness were marvellous, while in after-treatment he set an example for all to follow. Probably there have been few better writers on Surgery than Syme. It is true that he had no pretension to style, yet he compressed his meaning into sentences, which may be described as tabloids of surgical experiences. A writer in the *Lancet* after Syme's death said: "If our own Paget is the Cicero of surgical writing, surely Syme is its Tacitus." [1]

Syme had one failing, he could not agree with any one for any length of time, and so it came about that he and Liston quarrelled, and after 1823 they went their own ways; and the crowning conflict came when Syme defeated Liston in 1833 for the chair of Clinical Surgery in Edinburgh.

In a former chapter I have given Tait's description of Syme: how clean he was and how he was always washing his hands, and how successful his operations were because he was really the first of the modern aseptic surgeons.

Tait founded his ultimate practice on Syme's example, after he had been taught by Lister's work what to avoid.

In comparing surgeons with Liston, Syme, and Fergusson, Tait said that while Syme was called the Napoleon of Surgery, he had no dexterity of hand to compare with the marvellous dexterity of Liston and Fergusson, and he had no doubt that this was the real reason why Syme returned from London to Edinburgh in 1847.

Liston had died in an unexpected fashion, and Syme was offered, and accepted, the Professorship of Clinical Surgery at University College. He went to London and stayed six months, and then resigned and returned to Edinburgh. Syme gave some excuse; but Tait said that it was generally reported that Syme resigned his chair because his comparatively defective manual dexterity was so apparent in contrast to many of those with whom he had to be in rivalry.

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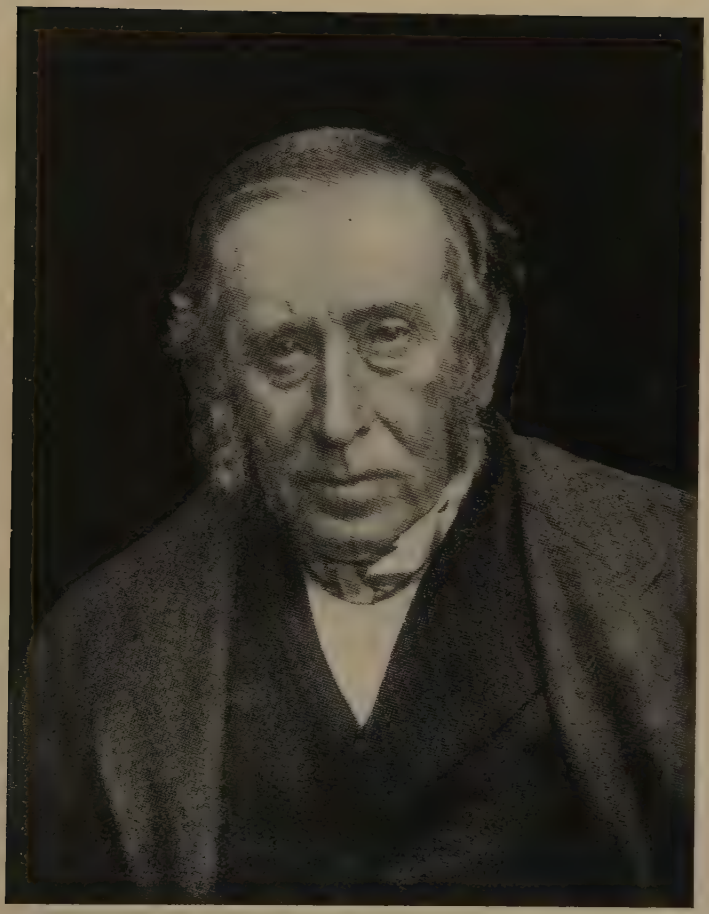
[1] *Lancet*, June 13, 1874, p. 841.

PLATE XXXI.



JAMES SYME AS TAIT KNEW HIM.

From a Photograph by A. S. Watson, Edinburgh.

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Tait said that Syme could never decently cut for stone to the end of his days, and but for the aid of chloroform Syme would never have left us the splendid philosophical principles of surgery which he laid down in the later years of his life.

Tait always impressed on us the great difference between operating in the early years of the nineteenth century and in the later years, and he quoted as a good example Syme's famous case of excision of the lower jaw for osteo-sarcoma—which was performed in 1828—when Syme was twenty-nine years of age. The tumour was the largest that had ever been removed by operation; the mouth was placed diagonally across the face and had suffered such monstrous distortion as to measure fifteen inches in circumference. Several surgeons had declined to operate on the case. Syme, however, was confident that he could operate with success. He seated the patient in an ordinary chair to prevent suffocation from hæmorrhage; the tumour was removed in less than twenty-five minutes, a little respite being frequently allowed to prevent exhaustion from continual suffering. Five weeks later the patient was quite well and thought of resuming his occupation.

Many of the operations performed at this period by Syme were done at the patients' homes, and this probably helped him, for if they had been performed in the theatre of a hospital they would have died of sepsis.

When Tait discussed the wonderful results obtained by Syme long before Lister introduced his antiseptic treatment, he used to say that Syme's results were due to a wonderful natural cleanliness. He was always spotlessly clean, his nurses were clean and he took good care to have no assistants' fingers in his wounds, as he did not know what their fingers might have touched before coming to the operation. Syme taught Keith his work, for Keith had first of all been Syme's patient, then his pupil, then his house-surgeon, and, lastly, his assistant, and Keith imbibed Syme's ideas on asepsis, and so made ovariotomy a success in the 'sixties. But Tait would not see that though these surgeons' results were excellent, and were apparently aseptic in a high degree, the difference between them and

the real aseptic surgeons of his own day and the present time rests in the fact that the *older surgeons did not know from what direction the real danger came to him*, he did not know, as Horsley said on one occasion, that it was not merely sufficient for him to wash his hands; he did not know that he had to exclude every avenue of entry of microbes into the wound—for the simple reason that he did not know that microbes existed.

Tait said that if Syme had not had a row with Lizars about ovariotomy he would not have taken up the stand against abdominal surgery that he did take up in the "thirties," and so he might have attacked ovarian cysts and his operative genius might have established the operation of ovariotomy long before that operation was a success.

Tait always spoke of Sir William Fergusson with respect and affection. He never forgot the kindly words that Fergusson had used when he presented him with the Hastings medal for his work on the "Diseases of the Ovaries."

There were many points in Fergusson's training that Tait admired and endeavoured to imitate. In his "Address on Surgery," delivered at Birmingham in 1890, Tait says that while the student was attending to the most important part of his training he ought to be taught how to use his hands, and he advised that he should be sent to the shop of the village carpenter, where he should be trained to use a saw, a chisel, a plane, and a skew, so that he should be able to make a long splint, if needs be, as well as put it on. And into the blacksmith's shop he should also go, till he knew how to strike properly with a hammer. Tait himself had done all this, and at his country home had a lathe and was quite an expert in its use. Tait in these matters had got some of his ideas from Fergusson, who, like Liston and Syme, was a Scotchman, and was born at Prestonpans in 1808. He at first entered a lawyer's office, then at seventeen exchanged law for medicine, and being attracted by the teaching of Knox, would often spend twelve to sixteen hours a day in the dissecting room. After taking his degree he gave demonstrations on surgical anatomy, and at the age of twenty-three he performed the operation of tying the subclavian artery, a

surgical feat which had been done only twice in Scotland. After marrying an heiress he shared the best surgical practice with Syme in Edinburgh, and in 1840 when thirty-two he came to London. There fortune favoured him, for within a few years such celebrated surgeons as Liston, Aston Key, and Astley Cooper died, and Fergusson very quickly stepped into their shoes and soon became famous. As an operator Fergusson was peculiarly skilled ; his manipulative dexterity was almost superhuman.

Like Liston he was a man of commanding presence, over six feet in height. Like Sir Charles Bell he was a splendid fly-fisher, and he was an excellent performer on the violin. Carpentering and metal working came easily to him, and gave him great readiness in improvising splints or other apparatus desirable for the patient's special circumstances.[1]

One of the points that every one who saw Fergusson always remembered was his superb coolness in the face of difficulty. Before he started an operation he had everything planned and worked out. His assistants were well drilled in every step of the operation. He never spoke ; there was no swearing if things went wrong ; everything usually worked out in perfect harmony. Fergusson's coolness was temperamental ; but it was helped by the fact that he was a master of detail, his forethought and vast experience enabled him to deal with the unexpected. In watching William Mayo operate the same description might be applied to him. He works like a machine ; he is never upset, his unique experience enables him to deal with every unusual point in the course of an operation, and he never loses his temper and upbraids his assistants for his own mistakes.

Tait was drawn to Fergusson because the latter was not in favour of vivisection ; and he admired him because he was a great patron of literature and the drama. Fergusson was, however, a much more generous man than Tait, and his spirit of hospitality was lavish whether in London or at his seat at Spittlehaugh. It is said that whenever he had any

[1] Bettany, " Eminent Doctors," vol. ii. p. 75.

consciousness of having done or said anything calculated to wound another's feelings, old or young, he never rested until he had made reparation in some way. Tait, I need not remark, never dwelt on this aspect of his character. Fergusson died of Bright's Disease in 1877, and Paget said of him : " He was the greatest master of the art, the greatest practical surgeon of our time."

If ever we mentioned Simpson's name Tait was always ready to say something in his favour. Syme had been his teacher ; Liston he only knew by report ; Fergusson he had had little or nothing to do with, but Simpson had been his friend, and was, I have little doubt, his father. Whether this be true or not, he always spoke of Simpson in terms of the deepest affection. He told us that he had gone to live at Simpson's home in Edinburgh soon after going to the University, and that he had acted as Simpson's assistant from the very first.

Simpson was born in 1811, and was the son of a small baker of Bathgate. In 1827 he entered as a medical student in the University, and became a dresser under Liston ; but he shrank from surgery, being of a very tender heart, and after seeing the agony that a patient went through while having a breast amputated he almost determined to give up medicine and take to law, but altered his mind ; determined at all cost to try and do something to make operations less painful. He quite early was attracted to antiquarian pursuits, and Tait inherited the love for the history of old things, including the history of the progress of anything connected with medicine and surgery ; and it was this incessant habit of directing my attention to what had gone before in the development of phases of surgery that first drew my attention to medical history, which culminated in my writing the " History of Ancient Gynæcology," which I dedicated to Tait's memory, for he died before the work was completed.

I asked Tait if Simpson had ever mentioned why he had selected obstetric work as his special study, as that branch of medicine in Simpson's young days was at such a low ebb that it was regarded with contempt. He said that Simpson had attended Hamilton's lectures, but had had no leaning to the

subject. Thomson, the Professor of Pathology, had, however, appointed Simpson to be his assistant, at a salary of fifty pounds per annum, and he was so impressed with Simpson's gentle manners, combined with a wonderful determination to get to the end of things, that he formed the opinion that his assistant would be the very man to succeed in midwifery practice.

Tait said that one of the secrets of Simpson's success in life was the power he possessed of making you believe in him. When he told you a thing you never doubted that it was otherwise than he had said ; and, as he had had a wonderful experience not only with women patients, but also with men, his charm of manner won you over after talking to him for a few minutes, and at once you had faith in him.

Tait often said that the only thing that has saved the medical profession from ruin was the sublime faith that the many patients have in their medical attendants ; but he held that this same faith has led thousands to their grave. A medical man performs some simple operation on a patient with success; she is grateful and recommends him to her friends; one of them happens to have some serious trouble and is advised to put herself in this medical attendant's hands, being assured by the patient who had been successfully operated on that " my doctor is a splendid surgeon ; see what a splendid advertisement I am for him." The doctor undertakes to operate on the second case, and she dies. Why ? Because while the practitioner was capable of doing small things he had not the training or the instinct of surgery. To be a surgeon you must be born with the *gift for surgery ;* just as a man is born with the gift for playing the piano, or playing tennis, or billiards, or many other things. Practice may improve an inferior surgeon, but the *great* surgeon is born, and has an instinct for surgery that is a gift. A good or a great surgeon is not a man who does one operation brilliantly and then fails in the next half-dozen. That is the life-history of the inferior surgeon. A great surgeon is a man who does operation after operation and has a long line of successes. The best surgeons meet with the most diabolic disasters at times. Tait said that few surgeons could afford

to publish the chapter in Paget's *Clinical Lectures* on " The Calamities of Surgery " : one would indeed need to be famous to live down some of the mistakes recorded there.

Simpson was no surgeon. He did minor operations, but he was a physician and shrank from any big enterprises in surgery. His work on acupressure was excellent ; but Syme, to show his contempt for it—as the work of a physician—had it destroyed before his class. Simpson was a ready and a good writer from the very outset. The first paper that he published of any importance was read before the Royal Medical Society in 1835 on the " Diseases of the Placenta." Immediately it was translated into French, German, and Italian ; from that date everything that Simpson wrote was received with attention, and later on with respect. Tait said that Simpson was one of the first modern writers to bolster up his papers with a great number of references. In his case, however, these references were genuine,[1] because he had consulted the original authorities ; he did not adopt the plan of some writers, who, having written a paper, direct their secretaries to copy out the references from the " Index Catalogue."

Tait said that when Simpson had been elected to the chair of Midwifery at the early age of twenty-eight, he had very little practice and no money, and in order to make both ends meet he sat up half the night writing for the press. Tait evidently inherited this trait from him, because when he came to Birmingham he had so little money that he could not pay the railway the freight on his books, and in order to supply himself with funds he became attached to one of the Birmingham daily papers, and wrote the leaders for many years, as I have mentioned above.

From the many stories told us about Simpson it was very apparent to me that though Simpson may have been Tait's father, the child did not inherit his parent's marvellous benevolence, and a generosity which made Simpson's name a byword in Edinburgh and many other places. Tait said that Simpson never bothered over a fee ; he would take one

[1] " One of his earliest papers includes nearly one hundred references to previous literature. If he could not read the language of an author he got some one who could to do so." (Bettany.)

guinea or five with the same grave kindly smile—he would slip the money into his pocket and forget all about it, and it was emptied out by his old servant when Simpson had retired, and next day it was banked. Tait said that one well-known story about Simpson was that once on a stormy night he got out of bed and looked for something to stop a window that rattled. Lighting on a piece of paper on the dressing-table he used it to stop the rattle of the window-frame; next day his wife discovered that the piece of paper was a *ten-pound note!*

Simpson, as I have mentioned, was a very soft-hearted man, and always tried to spare his patients pain, and having heard in 1846 about Morton extracting teeth in America after administering ether, and about Liston amputating a leg a few months later with the patient under ether, he determined to try and relieve the pain of labour with this anæsthetic. He succeeded; and to him must be given the credit of having first relieved the pains of childbirth by means of a general anæsthetic. It should be noted, however, that he first tried the effects of ether on himself; and Tait said that he was always quite willing to try any substance on himself that was sent to him with any anæsthetic properties.

Simpson was not satisfied with ether, and so he continued to hunt for other drugs that would not irritate the bronchial tubes in the way that the ether in those days did. He was recommended to try chloroform. He did not make chloroform. It had been discovered by Guthrie in 1831, and by Soubeiran in 1832, and by Liebig about the same time, while Dumas, after investigating the liquid made by Soubeiran, then found that Liebig had made an error in its composition. It was Dumas who gave the name chloroform to the liquid, and it was he who first worked out the real formula ($CHCl_3$).

Waldie, a chemist of Liverpool, when visiting Edinburgh in 1847, suggested to Simpson that he should try chloroform as an anæsthetic, and when Simpson consented to try it Waldie manufactured it for him at Liverpool.

Simpson tried it in company with Mathews Duncan and Thomas Keith; so that three gynæcologists are associated

with the first anæsthetic experiments of chloroform. Simpson was quite satisfied with the result of the experiments, and soon became convinced that this drug was the one that he had been seeking ; so he introduced it to the notice of the medical world in 1847, and soon it was used everywhere. Tait said that Simpson was the first to use it in obstetric practice, and he was soon condemned by the clergy on all sides for endeavouring to abolish the pains of labour, and it was declared that chloroform was " a decoy of Satan, apparently offering itself to bless women ; but in the end it hardened society, and robbed God of the deep earnest cries which arise in the time of trouble for help." Simpson entered this controversy [1] with his accustomed zeal, for though he was kind of heart, he did not spare an antagonist when the time came. Tait says that he had an idea that the controversy came to an abrupt end when it was known that Queen Victoria had had chloroform administered to her during one of her labours.

Tait said that Oliver Wendell Holmes—who was a personal friend—had told him that it was he who had first suggested the use of the word " anæsthesia " for the condition, and " anæsthetic " for the agent.

Tait many times alluded to the marvellous collection of patients that thronged Simpson's house from all parts of the world—no man of his time had such a practice—probably no man in the medical profession ever achieved so much fame as Simpson. Lister's name is universally known, but Lister's private practice was a poor one.

What Tait always marvelled at was the stupendous amount of work that Simpson was capable of doing day after day. He saw patients at his house, he attended midwifery cases, he lectured, he visited patients in Edinburgh, he was called in consultation to every part of Scotland and England, he produced paper after paper on many medical subjects, all showing much original research ; he wrote on acupressure and hospital mortality, and on many obscure antiquarian

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[1] Those who take any interest in this controversy will find it fully stated in the " Works of Sir James Simpson," vol. ii, 1871.

subjects; and yet with all this he was able to enter into controversies that would alone have sufficed to occupy the whole time of any ordinary man.

Once when we were discussing the origin of modern gynæcology Tait said that the Americans considered that McDowell was the founder, and if you objected to him they put forward Marion Sims, because his speculum and his "position" for examination were such great advances on anything else that had gone before, that they considered that those two things represented the real start of modern gynæcology. He himself was inclined to agree with this view; but he said that there were many who considered that modern gynæcology started with the introduction of Simpson's sound. I observed to him that when in London I had heard it suggested that gynæcology had started at "The Crescent," [1] Birmingham, and he laughed at the sarcasm, and said: "They made a mistake; it was abdominal surgery that started at The Crescent."

The last year of Simpson's life was spoilt by his family affairs and domestic worries, and not the least of these was the death of many of his children. It is strange to think that three of the four men—Wells, Morton, Jackson, and Simpson—whose names will always be associated with the introduction of general anæsthesia, should have had particularly sad endings to their lives. Morton died bankrupt and broken-hearted in 1868, before he was fifty years of age. A still worse fate befell Wells, who, having failed to establish his claims to priority on the discovery of anæsthesia, wandered the streets of New York until arrested for throwing vitriol. He was imprisoned and at once opened his radial artery and bled to death. Jackson the chemist, who first gave Morton the idea about ether and anæsthesia, died insane.

When Simpson died it was suggested that he should be interred in Westminster Abbey, but as he had expressed the wish to be buried near Edinburgh his wishes were carried out.

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[1] Tait's private hospital was in "The Crescent," Birmingham.

About Marion Sims, Tait spoke frequently and always with admiration.[1] He considered that as far as America was concerned Sims was the father of gynæcology there, if not the father of modern gynæcology. He said that the first time he met Sims was at one of the annual meetings of the British Medical Association. He was sitting on a bench waiting for the meeting to open, when he heard a man with a very pronounced Yankee accent say, " There's one man I have come here to meet, a young fellow called Lawson Tait; I want to have a talk with that young fellow." Tait thereupon introduced himself, and became a lifelong friend of Sims.

Tait said that Sims had a genius for inventing instruments for the performance of a particular operation; and he said that he told him that the well-known story about how he invented his duck-bill speculum was quite true.

This story has been told by Sims in his " Autobiography,"[2] which I read some years after I left Birmingham, and the story as told by Tait corresponds with Sims' written account. As it may not be known to every one it is worth recording on account of the important part that Sims' speculum has played in the history and development of modern gynæcology. That speculum was for a time the steam-hammer of gynæcology!

Sims told Tait that he invented his speculum by taking a pewter spoon and bending it, then fixing another and smaller pewter spoon to it. This accounts for the two blades of Sims' speculum being of different size; for Sims' spoons corresponded to our present-day table and dessert-spoons.

He was led to use the speculum after placing a patient on her knees who was suffering very considerable pain from " sudden version of the uterus backwards." Having placed the patient in position he introduced first one finger into the vagina and " immediately touched the uterus. I commenced making strong efforts to push it back, and thus I

[1] Tait dedicated the fourth edition of the " Diseases of Ovaries " to Sims in 1883.

[2] " The Story of my Life," Marion Sims.

turned my hand with the palm upwards, and then downwards, and pushing with all my might, when all at once I could not feel the womb or the walls of the vagina. I could touch nothing at all, and wondered what it all meant. When I placed my fingers there, the mouth of the vagina was so dilated that the air rushed in and extended the vagina to its fullest capacity by the natural pressure of fifty-five pounds to the square inch, and the pressure with the position was the means of restoring the retroverted organ to its normal place; then said I to myself, if I can place the patient in that position and distend the vagina by the pressure of air, so as to produce such a wonderful result as this, why can I not take the incurable case of vesico-vaginal fistula, which seems now to be so incomprehensible, and put the girl in this position and see exactly what are the relations of the surrounding tissues? "

On his way home Sims bought the pewter spoons at the store and converted them into his speculum, and then went to his private hospital and told the negress who had a vesico-vaginal fistula that he wanted to examine her. He placed her on the table in the knee-joint position, an assistant standing on either side to steady her.

> Before I could get the spoon-handle into the vagina the air rushed in with a puffing noise dilating the vagina to its fullest extent. Introducing the bent handle of the spoon I saw everything, as no man had ever seen before. The fistula was as plain as the nose on a man's face. The edges were clean and well-defined and distinct, and the opening could be measured as accurately as if it had been cut out of a piece of plain paper. The walls of the vagina could be seen closing in every direction; the neck of the uterus was distinct and well-defined, and even the secretions from the neck could be seen as a tear glistening in the eye, clear even and distinct and as plain as could be. I said at once, Why cannot these things be cured? It seems to me that there is nothing to do but to pare the edges of the fistula and bring it together nicely; introduce a catheter in the neck of the bladder and drain the urine off continually, and the case will be cured.

Sims, as all the world knows, set to at once and invented instruments necessary for performing the operation which

has made his name immortal among gynæcologists, and as long as surgery shall exist so long will Sims' speculum be honoured.

Tait used to allude to Sir Henry Thompson's operation on the Emperor of the French as being one of the best instances of bad luck in surgery that he could remember ; and he said that Sims told him another instance almost as bad. When Sims visited London he was asked to perform an operation on a case of vesico-vaginal fistula at the Samaritan Hospital. The case was a difficult one, but he said that the operation was in every way satisfactorily performed. But the patient died six days later, and a *post-mortem* revealed the fact that Sims had closed the ureters with the sutures. He said he had performed the operation hundreds of times and yet this was his first death !

To these examples of bad luck I might add one in Tait's career. He was asked to go to Rome in 1890 to operate on the physician of the Czar of Russia. He received a thousand guineas for the operation, and some private presents from the Czar. He performed the operation, removed some gall-stones, and the patient did well for some weeks, then died. Coming home through Florence he was asked to operate on an old lady in a desperate condition for an abdominal tumour. He operated on her, found a twisted ovarian tumour, and she recovered !

Tait was very fond of natural history, and thought that Gilbert White's " Natural History of Selborne " was a book that all boys should be made to study, for the facts that White records are still to be observed in England to-day as they could be in the quiet days at Selborne, a hundred years ago.

I think I made friends with Tait more quickly than I otherwise would have done, by referring, as an illustration to an argument, to the embryology of the lizard's brain. I had been but a few weeks at his hospital when he received a wire to go to Bournemouth. I accompanied him, and after an excellent lunch of grouse in a first-class carriage, which he had engaged, Tait became very amiable, and produced some of the vile cigars which he always smoked. We soon

fell to discussing various subjects, and he said, among other things, that he had delivered lectures for many years on biology and zoology at Birmingham. After discussing some biological subject, I said that the pineal gland could be explained when we knew that a pineal eye could be found to-day in many lizards in Australia; and told him that I had been the first observer to work out the embryology of this pineal eye.

Tait said, "What do you say? The *pineal eye?* I have never heard of it." Then I knew that I had him for once in my power, and so I gave him pineal eye *ad lib.* I will give him this credit, once you had him really beaten he gave in like a man. Several times after this I tried to corner him, but I was very seldom successful, except when I took the trouble to learn whole passages from Herbert Spencer's "Principles of Biology," and proceeded to lead him into an ambush. He had an immense admiration for Darwin, and was very proud that Darwin had quoted him in one of his books,[1] but he could not bear you to mention Spencer's name—one of his many prejudices that he would not or could not overcome.

Tait's love for animals no one could doubt. I have seen him at his country house take a cat up in his arms and carry it about for an hour, talking to it as though it were a child. I have said next to nothing in the first part of this book about Tait and vivisection, as it was a subject that I hardly ever discussed with him, because I knew that it would only tend to a bad feeling. I had not any sympathy with his opinions, which I knew from hearsay, and I could easily see that his kind heart would not let him agree to any experiments on the lower animals, even if these experiments were conducted under anæsthetics. He said on the few occasions that he touched on the subject that he had no doubt that men like Michael Foster, Burdon Sanderson, Victor Horsley, and such men, were never guilty of torturing dumb animals; but he refused to believe that all experimenters could be absolved from the charge, and he produced a small book

[1] "Variation of Animals and Plants," vol. ii. p. 452.

that had been sent to him to prove what he said. The book was written by Professor Wyman, and the title was "Abdominal Surgery." [1] He turned the pages until he came to the following, which I took the trouble to copy, because it was so shocking:

> The frequency of death after blows and kicks have been inflicted upon the abdominal wall makes it desirable to have more available knowledge as to lesions of the abdominal viscera caused by external violence than is commonly found in text books on surgery. Hence I give the following results of some of my own experiments, not that they may not have been repeatedly duplicated by others, but so far as I know through reading and observations similar experiments have not as yet been described in print.
>
> *Three medium-sized dogs* were made the subject of experiment. They were muzzled and tied standing on all fours. A burly negro coachman, with no fondness for dogs, was directed to kick with his heavy boots each dog three times in the abdomen. This he did, viciously and cruelly, as a most angry man might kick his fellow-man in a street brawl. No anæsthetic was given . . . .; the abdomen of one dog was opened 26 hours after the experiment and a rupture of the ileum was found, etc.

One could readily agree with Tait that any man who could be a party to such an experiment should be taken out and hanged on the nearest lamp-post; such a fellow is a disgrace to the human race.

> That, said Tait, is the sort of thing that goes on not only in England but in other countries, and that is one of the reasons why I oppose vivisection. The second reason is that I do not believe that the observations drawn from experiments on animals can be a proper, or a safe, guide to what occurs under the same conditions when the work is carried out on human beings.

And he proceeded to quote the enormous difference between the behaviour of the peritoneal cavity in the lower animals when subjected to infection and traumatism as compared to the peritoneal cavity of the human being. He said the country farmer could take a dirty knife and make

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[1] George Davis, Detroit, 1888.

an incision in a cow's side, just behind her ribs, and could insert his dirty hand into the peritoneal cavity and spay the cow, and the animal would show no signs of peritonitis nor infection. Having spayed [1] many cows since those days I know that what he said was correct.

Tait's objections are things of the past, and I don't propose to waste time in raising them from the dead. But I cannot let the subject drop without drawing attention to the avidity with which his opponents seized on the one sentence that he uttered in favour of vivisection.

Thus Victor Horsley, in his evidence before the Royal Commission on Vivisection in 1907, in speaking about the founding of the British Institute of Preventive Medicine, said :

> A meeting was held in 1893 in favour of it at Birmingham, and I was very keenly interested in the subject from a public point of view. I went to the meeting. Mr. Lawson Tait attended that meeting ; and the first resolution was, " That the members of the medical profession in Birmingham and district cordially approve of the objects of the British Institute of Preventive Medicine."
>
> Mr. Lawson Tait said that he fully assented to the resolution, feeling that while he objected to a certain class of surgical investigations, bacteriological experiments on animals had proved of great value.

These few words are used by Keen in his work on vivisection [2] to make it appear that Tait recanted. This is how Keen alludes to Tait's words : " He is constantly quoted by the anti-vivisectionists as a vigorous opponent of experimental research. In fact, he is the ' trump card.' But for evident reasons they never allude to his recantation six years before his death in 1899."

Stephen Paget, in his "Sir Victor Horsley," [3] makes the most of this single admission made by Tait, and Paget says on the strength of Tait's admission : " His position with regard to experiments on animals was equivocal."

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[1] Stewart McKay, " The Spaying of Cows," *Journal of Department of Agriculture*, N.S.W., May, 1894.

[2] " Animal Experimentation and Medical Progress," Boston, 1914, p. 11.

[3] London, 1919, p. 214.

Tait certainly made the admission, but to his dying day he still held tenaciously to his objections to vivisection.

* * * * *

There are some men—say, for instance, Darwin and Lister—who have an innate love for the truth, and whose aim in life is to arrive at the real facts in the course of an argument, and these men are so great, so splendid, that the discovery of the truth is of great moment, and affords them much satisfaction as the ability to prove that their opponents are in the wrong. I am afraid this was not the case with Tait. He wanted, of course, to arrive at the truth, but he preferred that the attainment of his object should be consummated over the dead bodies of his adversaries. This was not because he was unkind or cruel; in disposition he was a tender-hearted man, capable of infinite gentleness to man and dumb animals, but his nature was such that he preferred victory to be achieved with noise and bustle and with the utter discomfiture of his opponent; not in the gentle charitable manner that Darwin and Lister would have had it. Dr. Johnson once said, after publishing a pamphlet, that he did not think it had been a success, because his adversaries had not abused him nearly enough; we can quite believe that the same feeling came to Tait at times.

Away in the country and forgetting his contentions with his London opponents Tait was a very amiable fellow. He was fond of good food, was a great connoisseur of good wine, and thought he knew something about the art of cooking. One of his Birmingham friends told me that he was once at a small political dinner party where Tait and Gladstone were guests, and that Tait had told Scotch stories so wonderfully that Gladstone had been quite disconcerted, as he was generally considered to be *facile princeps* in that department of pawky wit. He would not stand low stories; he neither told them nor would he allow them to be told.

As far as sport went, he sometimes indulged in fly-fishing, nothing else.

He was very fond of music, and he was intensely fond of the theatre. He was a large shareholder in one of the Birmingham theatres and gave some personal attention to

its conduct. Every well-known actor or actress, from Henry Irving and Ellen Terry downwards, when they came with their companies to his city, would, of course, be invited to supper, and at these gatherings Tait was seen at his best. His epigrams and his wit were superb. I remember on one of these occasions that Miss Ellen Terry said that when walking down the main street of Birmingham she had encountered an old friend, who had acted with her for years. He had been famous for his enormous bass voice. She stopped and spoke to him, and he replied in a whisper. She, greatly alarmed, said: "But your voice, your great voice, what has happened?" He tapped the left side of his chest: "My left lung is gone," he whispered, "but my right lung is still here." The last sentence he had bellowed out so that it could be heard a hundred yards away. He had been merely pulling the great Ellen's leg! and she had loved the joke.

Tait was much attached to Toole, and he told many stories about his practical jokes. He said that he was one of the kindest-hearted men that ever lived.

There was one inconvenience about going to the theatre with Tait—he generally went to sleep and snored in such a disgusting fashion that every one within reach of him was ready to "brain" him. He usually did the same performance in a railway carriage, to the disgust of his fellow-passengers.

Tait took a great interest in all *objects d'art*. He possessed some rare gems, and his collection of Japanese articles was worth thousands of pounds. When he visited Italy he was able to obtain a poly-chromatic relief attributed to either Luca or Andrea della Robbia; but as the laws of Italy forbid that art treasures should be exported, Tait had to have his plaque completely covered by cheese before he could smuggle it out of the country.

His interest in art took a very practical form, because he was appointed Professor of Anatomy to the art students of Birmingham, and spent much time giving them lectures on anatomy as applied to art.

In the delightful trips we took at the week ends we spent

many happy hours in many different places. Tait's professional services being in much demand, he endeavoured to arrange to operate on the Saturday morning of the week-end, and so we would repair to the town or village on Friday afternoon. If possible Mrs. Tait always accompanied us, and after the operation was finished Tait arranged a driving tour for Saturday and Sunday. Mr. Hawkins-Ambler, a surgeon from Liverpool, who was staying at Birmingham at that time, often accompanied us, and we four had many delightful days together. The bigger the fee that Tait obtained the more amiable he was, but his insatiable greed never permitted him to pay his assistants' expenses. He insisted that we should pay our share, and once when driving through Wensleydale one of us happened to break the window of the hired carriage, he paid for it, but he sent us in the bill! He didn't even allow us the anæsthetic fee. When you visited his country home he treated you with the greatest hospitality, and allowed you to go to his cellar and choose any of the wine you took a fancy to, for he had some thousands of pounds' worth of choice wine stored there. On one occasion, when it was his birthday, he opened for me a bottle of champagne that the Czar of Russia had given him. It was some of the wine specially prepared for the Czar's coronation—the vineyard was at Gratz. It was very beautiful wine, but much too sweet.

When walking about a country lane Tait was at his best. With his dear wife's arm tucked under his own he would come to an old brick wall or a hedge or a churchyard, and then he only had to be " drawn " and he would begin.

Some of my notes tell me that on one afternoon he discussed the paleontology of the material in the Roman walls in England; the reason for growing yew trees in churchyards; why a cuckoo lays her eggs in another bird's nest; the exact spot where William the Conqueror landed in England; and a long dissertation on how the old Barons got their supply of wine to their country houses from the coast, when the same wine had to pass through one country after another and encounter the bands of bushrangers that infested rural England in those days of old.

On one occasion Tait explained how the young cuckoo got the young of its foster-parents out of its own nest. He said that the young cuckoo, even before its eyes were open, and before it had any sign of feathers, would set to and gradually get its back under one of its nest mates. Then it climbed backwards up the side of the nest and jerked the young one from its back over the edge of the open side of the nest. Tait further explained that the egg was not laid in the nest but conveyed to the nest, being carried either in the mouth or in the throat of the cuckoo.

Once at his country house in the evening Tait drew my attention to the note of a nightingale in a tree in his grounds, which, he said, rang out as though he were singing, " Sweet, sweet, sweet, sweet, Jug, jug, jug, jug, jug." All that I remember is that I heard for the first time a note like as if it came from some huge canary, and the volume and purity of the sound surprised me very much. I could hardly imagine that any bird could have produced such a wonderfully full rich luxurious note.

After hearing this bird I told him about the way that the Australian lyre bird mimics other birds, and will even imitate the sound made by the saw of the bush saw-mill. Perhaps there is no bird in the whole world that is such a perfect mimic as the lyre bird.

Sitting round a table in one of those delightful inns that look so picturesque outside, but which, alas ! contain only beer and bacon and eggs inside, we would be quite content to have a simple meal. Tait would sometimes have what we used to call a *significant-etymological-fit*, and would tell us the origin of the names of every article on the table.

He loved to confound us by telling us that a spoon and a shingle were related, for the original words are alike, and the first spoons were but chips of wood ; and that a spoon and gallantry is in German the same word, " toffel," hence our expression of a pair being spoony. He did not fail to point out that cochlearium, the spoon of the doctor, is related to cochlea, a snail's shell, because the first spoons were used to extract snails from their shells.

Luncheon or lunch, he said, was connected with lump, and

originally signified a lump of bread: for that was all the meal consisted of.

Whisky is derived from two gaelic words, which mean the breath of life! while brandy meant burnt wine. I always remember his explanation of cutlet: this is the same as chop-let, and the German is cotelette, and the French côtelette, and these words go back to the old French costellette, cote being rib, old French coste, and Latin costa, a rib, so we get to cutlet, a little rib cut.

I asked him one day what punch was derived from, and he said that it could be traced back to the Sanskrit pants-chan, which meant five, because it was made of five ingredients—spirits, water, lemon-juice, sugar, and spice.

But nothing that Tait explained to me ever surprised me so much as some of the details of Phallic worship. One day he went some miles out of our way so that he could show us a window in a very old tumbled-down church. Its oval form, he explained, was that originally used as a symbol of the external female sexual parts: that is, the vulva. It is often referred to as the "yoni," as that is the term for vulva used in India. He told me that he had once seen a print of a picture of the Resurrection, in which Christ is painted by Raphael surrounded by a yoni. Still more did he surprise me when he pointed out that the Cross was a sign that in the first place represented the male organs of generation. Man by his strength was supreme over woman—he was the father of the family; according to Anaxagoras, an old gentleman who lived three or four hundred years before Christ, the embryo was formed from man's seed—the mother merely supplied the resting-place. Thus man was the father, the creator of the family. His external organs of generation—what the Greeks called the *phallus*—were taken as the symbol of his power, and the symbol in phallic worship in time came to be used as a symbol for the Creator himself. Tait was at pains to point out that the cross that we so often see now—the Latin cross—was not the same as the tau cross (T), which was probably the actual shape of the cross on which Christ was crucified. If we turn the tau cross (T) upside down we

find that it has its origin in the Greek phallus(♃), the external organs of generation of man. He laughingly said that the arrow of Cupid was the same thing; the two spikes and the centre rod denoted the same object as the Greek phallus.

Once when we came to a village in which they were holding a village festival, he said that in the old days in Sicily they held Bacchic vintage-festivals, and bands of jolly companions went about in carts, carrying the phallic emblem. These holiday makers sang lewd songs and were called comus-singers. The greek κῶμος meant a festival, and also a carousal, and was supposed to be derived from κωμη (a village); others said, however, that κῶμος was a wine banquet at which the guests reclined; all this ended in his saying that this phallic festival gave rise to the word *comedy*.

Tait had considerable mechanical genius. Like Doyen, he was a skilled worker at the lathe. He loved plain instruments, and this led him to invent simple instruments to take the place of complicated ones. In his younger days he had introduced an elaborate trocar for ovarian cysts, but after a while he threw this aside and never used anything but the simple "ascites tube," which we have all found so easy to sterilise and keep in order.

He would never use a curved instrument if a straight one would do, for he said that when you used a curved instrument you never knew where the distal extremity was when it passed out of sight.

The following are some of the instruments that he introduced and which were called after him. Chief among them are his artery forceps, the ascites tube, ovariotomy trocar, hysterectomy serre-nœud, uterine dilators, repositors for inverted uterus, suction syringe, glass drainage tubes, corkscrew for fibroids, gall-stone forceps, gall-stone scoop, écraseur with rope to use as a temporary clamp, pessaries, plain glass speculum of the Fergusson type, and special scissors used to perform his perineal operations, and the electric cautery for hæmostasis of the broad ligament. Lastly, his most important invention was "Tait's hospital bedstead," whose use has become universal.

XXXVIII

Tait: the Man and the Surgeon

I have given in the preceding pages an account of Tait's contributions to surgery, and I have given glimpses of his private life; the time has come when I must gather up these disconnected lines and try to weave an estimate of Tait the *Man*, and Tait the *Surgeon*.

Let us consider in the first place Tait the man, and see how the man was father to the surgeon.

I have given my reasons for thinking that it was quite possible that Tait was the son of Sir James Simpson. His marvellous physical resemblance to Simpson; his massive head; his love of controversy; his passion for writing to the papers; his never-ceasing industry; his close intimacy with Simpson when quite a lad; his living with Simpson after he became a medical student; the handing over to him of some of Simpson's papers after the latter's death, so that Tait might continue his master's work, are all facts that make one inclined to think that there was something in the report that Tait was a natural son of the great Simpson. But if the idea was false it matters not. Tait was not responsible for his forbears, and, like Napoleon, could say, "I am the ancestor."

I shall first deal with his physique.

I do not know why I should have had the idea before I saw him that Tait was a tall graceful man, but that preconception was in my mind, and when he came into his library at The Crescent and asked me if I wanted to see him, and I looked up and saw a short, broad-chested man in a velvet coat with a monstrous head from which fell long hair, and with a face that was severe and plebeian, I was so flabbergasted that I did not realise that I was in the presence of the great surgeon.

Personal magnetism in a medical man is a difficult thing to define, and a still more difficult thing to explain. Some men have good looks, others are almost repulsive in countenance—the late Sir William Jenner, for instance—some men, like Sir William Gull, overawe you. Tait told me that whenever he spoke to Gull he always felt that he was in the presence of a great personality. Some men have a harsh, almost brutal manner, others have a way that will " charm a bird out of a tree " ; some men impress you with their marvellous memory —no patient appears to be forgotten—while another has the art of making you believe that for the time being you are the only patient that he has in the world.

Lauder Brunton once wrote :

> I believe that a good deal of the influence which certain medical men have upon their patients is due to their powers of suggestion. You sometimes meet with men who may be really ignorant, but are nevertheless most thoroughly convinced of their own knowledge and of their own superiority ; you may find that these men are not only more acceptable practitioners, but are perhaps even more successful in their treatment than other men who are very much better up in their profession. Their patients believe in them, take the medicine they prescribe, whatever it may be, and the result they have expected follows.[1]

I don't know which of these " ways " applied to Tait. You saw before you at your first visit a thick-set man with an enormous head, and a face that was not good-looking—it was severe in its expression—and you realised that you were in the presence of a man who took life seriously ; he seemed to have some hidden power, some reserve of force on which he could draw in the hour of difficulty and danger. Tait's face, I think, could best be described as grave. Many professional men are grave ; they listen to what you have to say, they nod their heads like a Chinese ornament, and they impress by their gravity. When you spoke to Tait on a serious matter he was grave, and for this reason I have chosen from a number of pictures that I have of him the one that is the *frontispiece* to this book, because it represents him as I often saw him in 1891. Gravity in some medical

[1] " Lectures on the Action of Medicines," 1897, p. 231.

men is a mere cloak for ignorance, or, as Rochefoucauld puts it, *gravity is a mysterious carriage of the body invented to cover the defects of the mind.* But this was not the case with Tait; he listened to what you had to say, then what he had to say he put forth in as few words as possible, his lips hardly moving.

If you were arguing with him his gravity meant that he was waiting his opportunity; when you were finished he asked a few unimportant questions, and when you thought that you had converted him he turned in a moment and ran you through with a rapier-like thrust; after which he would beam on you. He spoke in an incisive way, and he put a matter before you so clearly that you came to the conclusion at once that what he said was probably correct. It is this power of the medical man to inspire confidence in their patients that has saved the medical profession from bankruptcy; at the same time it has sent thousands to their grave.

One had not spoken to Tait for very long before one came to the conclusion that he was a "man of common sense." Never for one moment would you estimate him as a visionary; one who would have you believe that mere theory is science. One soon formed the idea that he resembled the man that Pearson has told us of—one who could present you with such a classification of facts that your mind came irresistibly in its process of thinking to acknowledge a logical sequence, and at the same time the thought would come that he was one who would appeal to your reason before he captured your imagination.

Tait had common sense, and common sense, as science does, rests on information and interpretation, and is the product of reason and judgment with a special facility for applying them to facts and conditions as thay are seen.

To me it always appeared that Tait's *common sense* was the thing that struck you sooner or later.

Doran said that Tait was not scientific. The scientific method of deduction is more cautious and more restricted in its generalisations than personal judgment may be; but so far as it goes there is nothing antagonistic in it to good

common sense. Tait, as Doran said, "was no man of science like Huxley, the pure scientist, or Paget, the surgeon, who knew how to apply science to surgery." We may grant that Tait was not like Huxley, because the latter was the very highest type of a scientific man. Huxley sought after truth, and he sought according to precise methods. He recognised that theory and hypothesis were necessary parts of scientific investigation, but he was ever careful to point out that pure theory was not science, nor is science mere theory. The scientist of the Huxley type finds out the unknown in nature through the known; but Tait was inclined, as Doran has quite justly pointed out, to make scientific facts appear to support his personal views and justify his practices.

I don't think that Huxley would have appreciated the term *pure scientist* when applied to himself, because he used to hold that people spoke of a sort of scientific knowledge of direct practical use called by them *applied science;* while there was the other sort of scientific knowledge which is of no practical utility, and which is termed *pure science.*[1]

The comparison between Huxley and Tait is hardly fair. While we must admire the great work that Tait carried out, it was necessarily imperfect in many respects. We all know that many of the observations that have been made, and have served as foundations upon which medicine and surgery have been built, have proved to be only sandbags that have failed to bear the pressure of modern investigation. Huxley worked at a branch of science—zoology—where he was able to deal with the concrete. By almost superhuman labour he had examined the details of this branch of science and had found colossal errors on every side—errors of mere observations and wrong conclusions—and he was able to sweep away these accumulated errors in his masterful way. But the branch of science—if, indeed, we may dignify it as a branch of science—in which Tait laboured was not amenable to such a process, and consequently it was not capable of being checked by the exact methods that could be applied to zoology. Had Tait been a professor of biology, instead of a mere builder of

[1] "Science and Culture: Essays," vol. cxi. p. 156.

gynæcology, I have no doubt that he would have risen to a level where his scientific acumen would have earned honourable appreciation. He, in the course of his building-up work, had of necessity to alter from time to time tentative opinions, because he did not possess sufficient data to complete his observations; but this occurs in the history of the work of all men who are finding their way through the labyrinth of a new region of knowledge.

I am sure that if Tait and Huxley had been thrown together, and had come to know each other intimately, each would have appreciated the other's work, for Huxley was a master when he came to deal with great problems of life, and he had the power of crystallising available knowledge on an obscure subject, and of presenting it to you in a form that even if it were imperfect, it was so splendid that the ordinary man could find little to cavil at. I have no doubt that he would have objected to Tait's unscientific opposition to the germ theory and to vivisection; but he would have had a great admiration for what he had done in original work—work that had brought so many benefits to suffering humanity; and Huxley would have taken Tait's hand and stood by him—just as Goethe stood by Schiller at Weimar—for he would have recognised in Tait a promoter of that freedom of thought, which he proclaimed from the house-tops, as at once the cause and the consequence of intellectual progress.

But Tait exhibited a condition of mind which Huxley would not have approved of. This condition I may best describe as *prejudice*. Prejudice is considered as rather a common commodity in the "brain market," and yet it is one of the most active factors in the psychology of the thinking world.

Prejudice may be the result of reasoning, or it may be the immediate result of a brain condition that is loosely described as *instinct*.

If we suppose that the brain works in two ways, and we define those two ways as the *objective* and the *subjective*, then we shall have no difficulty in believing that prejudice is most often a result of the *subjective* working of the brain.

Let us therefore try and get to the root of prejudice in order that we may better understand the man Tait.

PLATE XXXII.

HUXLEY.

To face page 532.

A close study of his mind convinced me that Tait was a man largely influenced in some of the essentials of life by a dominating prejudice. If we ask ourselves, "What is prejudice?" we must answer that it is a preconceived notion arrived at before we have dealt with our final conception by observation, comparison, abstraction, and generalisation.[1]

If our object is to arrive at the truth, we must attack a problem with an open mind, and try to lay bare fact after fact. We can next assemble these facts and marshal them, and they will then be ready for us to form a basis for accurate judgment. But the man who attacks a problem and allows preconceptions to influence his findings—most preconceptions arise from imperfect data—will, as a rule, draw wrong inferences, being prejudiced by his preconceptions, which blur the clearness of his judgment. Such men always find what they want to find. Demosthenes said, "What we wish that we believe"; and Aristotle said, "What we expect that we find." What is the consequence of prejudice? Should the facts observed be contrary to our preconceived notions—self-love defends them to a point of honour, exaggerates whatever may conform, overlooks or extenuates whatever may contradict, and the result is that the truth is not reached, because the real phenomena, if admitted, as Sir William Hamilton used to say, would spoil the pleasant music of the thoughts and convert its factitious harmony into discord.[2]

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[1] Darwin, when considering the *struggle for existence* and the *formation of new species*, tells us: "Here, then, I had at last got a theory by which to work; but I was so anxious to avoid prejudice, that I determined not for some time to write even the briefest sketch of it."

[2] "Lectures on Metaphysics and Logic," vol. i. p. 75. At a meeting of a London medical society, to which Tait had taken me, a well-known surgeon got up and made a speech in which he asked his fellow-members not to take too much notice of the views expounded by Tait, because every one knew he was a man of strong prejudices. The other members spoke, and then it came to Tait's turn to reply. He took no notice of the criticism of the other members, but he happened to be seated behind the old gentleman who had accused him of being prejudiced. Tait stood with his hands resting on the old fellow's chair, and said: "Gentlemen, I have been accused of being prejudiced; let me assure you that I have only one prejudice in this world, and that is against a man who *dyes his hair*." He sat down, and a roar of laughter arose, for every one in the room knew that the old man had for years dyed his hair with the same elaborate care that Sir Henry Thompson used to practice.

Still dwelling on the subject of prejudice, one may recall the saying that "long continued practice solidifies opinion, and makes it impregnable to evidence, and we come at length to substitute habit for reason." I think these words can be applied to Tait, when we regard the attitude that he took up towards the germ theory and vivisection. He had got certain ideas about germs into his brain, and they had solidified his opinion and made it impregnable to evidence. This is certainly not the proper spirit for one to assume who wants to be considered a scientific man. Whatever Tait's attitude towards germs in their relation to diseases may have been in his younger days when the evidence was imperfect, he was not justified in keeping up this attitude in later years, because the development of facts with regard to bacteriology had been the outcome of genuine work in science. There in truth had been a presentation of facts which had been gradually classified, until they irresistibly forced the prepared mind to acknowledge that these facts led to a perfectly logical conclusion. Koch's work alone placed the germ theory on a scientific footing, and this should have satisfied any man who was not hopelessly prejudiced; and this, I must admit, Tait was when he discussed germs and their causal relation to wounds and infection.

If, then, I have to admit that prejudice clouded Tait's outlook, we must not let this fact cast too great a shadow. His prejudice against vivisection did little harm—those who had real scientific work in physiology at heart went steadily on, and looked with pity on the man who could so blind himself that he refused to acknowledge the great benefits that human beings had derived directly, and indirectly, from experiments on animals. As regards Tait's prejudices to *Listerism*, it really had no bad effect on his own surgical work, and in the end it proved to be the starting-point which the present-day *aseptic* technique had its origin; but we must not forget that the surgeon of to-day knows all the grounds that this aseptic technique is founded on; and he also knows that while Tait's practice was very successful its theory was founded on false premisses, because it did not proclaim the *absence of the germ* as its keynote; he contented himself

with saying that *scrupulous cleanliness* was the secret of his success.

After prejudice one of the outstanding traits in Tait's character was his *boldness*. No one could accuse him of a want of courage, and this courage led him to attempt operations never performed by any other surgeon. He has told us that he hesitated to open the abdomen in a case of ruptured ectopic pregnancy, and the patient bled to death; then, when too late, he saw that he had missed a great opportunity, and that taught him a lesson that he did not forget, for the next time he did not hesitate: but he was not successful: this did not frighten him, for when the opportunity came again he carried out what he had determined to do, and this time success crowned his effort; then he proceeded to operate on case after case, and all successfully.

He did not hesitate to attack the gall-bladder and the hydatid cyst, and his work on pelvic abscess and pus tubes will always command the admiration of those who know that these pathological conditions, even to-day with all our accumulated knowledge, are among the most formidable conditions that the expert of us may be called upon to deal with; and we fail at times now, and we are liable to fail in dealing with these conditions for many a year to come, unless we gain some knowledge that is not in sight at present. Any progress we have made beyond the point where Tait stopped has been due to prophylaxis.

Doran, in speaking of Tait, said:

> Boldness and dexterity were his strong points. Through boldness he established operations such as cholecystotomy, and valuable aids to the operator, such as cleansing the peritoneum. True, he acted in most cases on suggestions derived from others; but we must not neglect the claims of a man bold enough to force on surgery necessary innovations which others have introduced, but have failed to establish through lack of certain qualities and opportunities.[1]

This is a fine estimate of Tait by Doran, because we must not forget that Doran had been the friend, the assistant, and

[1] *Trans. Obstet. Soc.*, London, 1901, p. 69.

the colleague of Spencer Wells for a lifetime, and we know that Tait and Spencer Wells were very bitter enemies.

Those who will consider Tait's claim to greatness with fairness will always respect Tait's courage, because his courage was not the dashing of a tyro into an unknown field, the outcome of which was death to the patient. His courage was rewarded again and again by success, and each success made him more confident in his powers and incited him to attack fresh fields: thus was modern abdominal surgery founded and extended by him. He was the true pioneer of *Pelvic Surgery*, or, as William Mayo says, "The father of modern abdominal surgery."

Tait was fearless, but he was not reckless. Human life was very dear to him, and as the years passed by his skill became consummate, because his experience in abdominal surgery was greater than that of any surgeon who had lived before him, or who was living during his active period of work: for had he not performed four thousand abdominal sections? at that time a unique performance. This high total has been surpassed by a few men like the Mayos, who can now claim a unique experience in the number of cases operated on in this department of surgery. But Tait and the Mayos differ in one respect: Tait had to blaze the way, he was the pioneer, he pointed out the road, and the surgeons of the world have followed. This fact alone will make him famous for all time.

I think Tait's activity could be best expressed in the words of Homer, *ἄμοτον μεμανῖα*, an insatiable raging! and a critic has said of him:

> His restlessness of disposition and tenacity of purpose led him into association with most of the public movements during his residence in the town (Birmingham), in all of which he was found to be a dangerous opponent and an aggressive champion of the cause which he took up. In his life of constant struggle he made many friends and also many foes, for he never took trouble to conciliate those who differed from him, with relentless criticism continuing to fall upon his adversaries with heavy hostility. It was difficult to cross swords with him in controversy, as he depended less upon the facts than upon the object which he had in view for a justification of anything he might say. Hence his methods often

led to avoidance of antagonism on the part of others. But, as a rule, Tait's adversaries were ready to allow to the doughty antagonist full credit for courage and for a fine desire to fight on the side, as he conceived it, of the weak and the oppressed. He dearly loved a losing cause and a concensus of accepted opinion was anathema to him. Had there been a seriously divided opinion in the medical profession upon the question of the scientific value of experiments on sub-human animal life, Tait would have been less ardent in the cause of anti-vivisection, and more keen to sift the statements and weigh the alleged arguments of the non-scientific persons who welcomed him so warmly to their ranks. Had Gladstonian Liberalism stood a practical chance at Birmingham half the charm for Tait in contesting for the honour of the Parliamentary representation of the Bordesley division would have been absent. Had any other surgeon possessing a tithe of the position or professional skill thrown doubt on the value of the antiseptic methods Tait would have been far less ready to appear as a public sceptic, even though his own rigid attention to cleanliness and technique gave him excellent results.

After reading the above one is inclined to apply to Tait the words of Goethe, " We may not in all ways agree with extraordinary men, but there is no reason why they should not have our love and admiration." I quite agree with an unknown writer that Tait dearly loved a losing cause. If he thought he was right, or a friend was right, he came forth and proclaimed this belief. He knew that what George Eliot has said was true, " Truth has rough flavour when we bite it through." He did not fail to bite, and if he occasionally broke a tooth in his effort it did not deter him. The younger members of the profession had a great admiration for him, and on any occasion when he thought any of them had been unjustly attacked he was ever ready to come forward and help them ; for this reason his evidence in any lawsuit was eagerly sought after, and his attitude was often so aggressive that he had to bear the burden for months after the case was finished. This was well shown in the famous case at Liverpool when Imlach was sued for his operation on the Fallopian tubes.

Tait liked to have young people about him. He seemed to feel very much the absence of children from his home, and expended his affection on animals and young people. I

can mention two examples of his kind heart: the first was in connection with a passage in "Rab and his Friends," and the second concerned myself.

With regard to "Rab," Tait was a great admirer of John Brown's masterpiece. He had known the author when a student at Edinburgh and told me that the surgeon who operated on Ailie was Syme; and Tait would take the book down from the book-shelf and open it and read us the account of the operation; then when he came to where the patient got down from the table after the operation without anæsthesia his eyes would fill with tears when he came to this passage: "It is over: she is dressed, steps gently and decently down from the table, looks for James; then, turning to the surgeon and the students, she curtsies—and, in a low, clear voice, begs their pardon if she has behaved ill. The students—all of us—wept like children."

Tait said that this picture of sublime suffering without a murmur always affected him for it was the very thing that had occurred in the days before anæsthetics, and it was such scenes as this that had wrung the kind heart of Simpson and made him eager to obtain some drug that would ease the sufferings of these unfortunates.

The second instance of his kindness was shown to me, when, after returning home to Australia, I wrote and asked him to send me a few lines that might help me, as I thought of applying for the Lectureship in Gynæcology at the Sydney University. By the first mail my kind-hearted teacher sent me the following letter:—

My dear Dr. McKay,

In the event of the establishment of a Lectureship in the Diseases of Women at Sydney, I know of none of my pupils,—not one,—who I could so well recommend for it as you. You have a splendid scientific and general medical training. You have all the personal requirements of a first-rate surgeon, and your enthusiasm in the special line of Gynæcology showed itself, when you were under my tuition, as above all praise; and if it is any satisfaction to you to know, it gives me great pleasure in telling you that I consider you the most promising pupil I ever had.

To many who may read my letter this praise of you may seem overdrawn, but it is not so; whilst those who know

At Home.
Mr. Lawson Tait. 12 till 2.
Mr. Christopher Martin. 11 till 2.

BIRMINGHAM.

Feb 24/23

My Dear Mackay,

I am not in the least surprised. I don't think you are bound to give any reason & it will be wise not to do so. Through the temptation is great. Yours ever
Lawson Tait

SHOWS TAIT'S HANDWRITING AND THE PAPER HE USED AT BIRMINGHAM. THE SEVEN STARS MEAN "7, THE CRESCENT."

To face Plate XXXIV.

TELEGRAPHIC & RAILWAY
ADDRESS:—
"LYNDHURST ROAD.
HANTS".

Ruskett-Fletchwood.
Totton.
Hants.
(For Postal purposes only.)

Apr 7/92

My Dear Mackay,

I am down here till Monday night. Let us go Saturday & Sunday & have a long Drawings will do, I think. Remember drawings early. Send prospectus

Yours Tait

LETTER WRITTEN FROM TAIT'S COUNTRY HOUSE IN THE NEW FOREST.
It shows that he took no risks as far as his address was concerned.

To face page 538.

how rarely I give testimonials will recognise it at once as evidence of your most exceptional merit.

Yours ever,

LAWSON TAIT.

Birmingham, 1893.

Tait's favourite method of explaining his views was of course by writing papers and letters to the weekly papers such as the *Lancet* and the *British Medical Journal*, and while he often deeply wounded an opponent, he did not object to the thrust from the other side: we may say of him, as Richard Cumberland in his ode said of Johnson:

"In controversy seldom known to spare."

Garrison, in his "History of Medicine," has summed up Tait in the following words: "He was a forcible, effective, frequently coarse, but always amusing, writer."

So much for Tait the man: as a surgeon—

> Tait was, indeed, a rapid and brilliant operator, quick in judgment, fertile in resources, and apt in dealing with difficulties. Not giving so much profound attention to diagnosis, which he knew and felt was often problematical, he solved the question of abdominal operations by his insight into the pathological conditions present, and by bold and often daring procedures he met with results which signalised his skill and power of overcoming difficulties and complications.
>
> Self-confident and capable, he was always aware that his strength was exhibited in the face of danger, and he inspired the feeling in those with whom he was associated that fear was unknown to him, and that nothing was ever done by him in response to so unreasonable a sentiment.

So wrote some one in the *Lancet* after his death.

As regards his rapidity of technique at an operation, it was marvellous. As I have mentioned above, August Martin, of Berlin, would begin and finish an abdominal section for a pyosalpinx in eight minutes. Tait was hardly as rapid as this, but he was "next door" to it. His small opening in the abdominal wall, his uncanny left index finger, the almost superhuman dexterity of those short thick-set hands, that seemed more fitted for a carpenter than a surgeon, helped towards one of the most wonderful exhibitions that could be seen in the practice of surgical technique. I suppose that the only man in Europe that

was more dexterous in surgical technique than Tait was Doyen, who, I think, will readily be acknowledged as the champion surgical acrobat of modern times. Liston could amputate a thigh in twenty seconds: Fergusson could cut for a stone in eight seconds: Martin removed a double pyosalpinx in eight minutes; but Doyen could do a vaginal hysterectomy in two minutes! I have often seen Tait perform his famous perineal operation for complete rupture of the perineum in five minutes!

Tait's rapidity was the outcome of perfect technique; hurry was conspicuous by its absence; he saved time because he saw at a glance what had to be done, and he was able to do it with a master hand. There are many surgeons who know what to do but who cannot carry out their work in a proper way for want of manual dexterity. To them we may apply the well-known words of Claude Bernard: "A dexterous hand without a head to guide it is a blind tool. A head without a hand to realise its wishes, is an impotent nothing."

It may be truly said that Tait achieved speed, his dexterity and knowledge led him; he did not aim at speed—he simply achieved it.

Tait and Conflict.—Tait's life had been passed in conflict. Some of this was inevitable, but some he could have avoided, but would not: he courted opposition, for he maintained that opposition was the essential stimulus to make you fight and produce what was best in you. No better example of what Tait was capable of could be quoted than the incident that I have mentioned in one of the earlier chapters, when he found that the qualification for the office of Surgeon to the Hospital for Women, about to be founded in Birmingham, was fixed at the Fellowship of the London College. As he had but a year before become a member, he set to, and working day and night acquired the requisite diploma. A writer in the *British Medical Journal* says that "the late Mr. T. H. Bartlett, who was closely associated with him at the time, was lavish in his praise as to the ease and rapidity with which Tait prepared himself for the examination."

While he loved opposition he warned us never to attempt anything that was obviously beyond our powers. He had some pet phrases such as, " Don't try and cut bricks with a razor " : " Don't break a vase for a shy at a rat " : " Don't try and stick a rhinoceros in the hoof with a fruit knife." But he often quoted Rochefoucauld, who maintained that few things are impossible in themselves ; *application* to make them succeed fails us far more often than the *means*.

Charles Reed, the American surgeon, has said of Tait that " criticism was to him a wholesome stimulus, and antagonism but an incentive to further endeavour," and we can easily imagine Tait at this period as one who was " yearning for the large excitement which the coming years would yield."

Tait as a Speaker.—We might almost apply to Tait old Ben Jonson's quaint description of his friend Francis Bacon's way of speaking : " No man ever spoke more neatly, more freely, more weightily, or suffered less emptiness, less idleness, in what he uttered. His hearers could not cough, or look aside from him, without loss." [1]

Operations on other Medical Men.—Tait liked other medical men to come to him to be operated on, and he always let that fact be known. In 1889 a well-known American surgeon came to him and he operated for gall-stones. At the Birmingham branch of B.M.A. the surgeon read an account of his case. He said :

> The operation was performed by Mr. Lawson Tait. A vertical incision, four inches long, was made directly over the gall-bladder, which was almost entirely hidden by the liver. The bladder was stitched to the parietal wound, and over a hundred stones extracted. On the fifth day the stitches were removed, on the eleventh day the patient was out of doors, and on the fourteenth day only a small fistula was left, from which a small quantity of bile-stained mucus exuded.[2]

Tait was very pleased when asked to go to the Continent to operate on Professor Budin. In an obituary notice M. Budin thus refers to his own case :

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[1] Ben Jonson's " Timber."

[2] *Brit. Med. Jour.*, December 14, 1889, p. 1341.

> He (Tait) was one of the first to undertake with success surgical proceedings on the biliary ducts and the liver itself. That is why, in 1885, on the advice of Professors Bouchard and Tarnier, we asked him to be so good as to go to the south of France to perform on us a laparotomy for an obscure hepatic disorder. On Monday, May 4, he received our letter at noon; on the 5th, he left Birmingham, met Tarnier when passing through Paris, and reached Beaulieu at 8.30 a.m. on the 7th. At 11 a.m. he operated on us in the presence of our friends Drs. Bar, Crouzat, and Thaon (of Nice). He brought with him Mr. J. W. Taylor as his assistant. It is needless to say how much gratitude and affection we felt for him; the news of his decease plunged us into deep sorrow.

Budin never forgot Tait's kindness to him, and when he published his now famous "Femme en Couches et Nouveau —nés" he dedicated it to Tait in these words: "Â Lawson Tait: Grâce à vous j'ai pu écrire ce livre. Permettez-moi de vous le dédier. Votre très reconnaissant Pierre Budin."

Religion.[1]—Tait was not keen to give you his views on religion, unless you asked him directly. He gave me the impression that he had a distinct leaning towards the Roman Catholic Church. He said *that* religion was a hard one to live up to, but an easy one to die in. One day I quoted to him Carlyle's words, "That a man's religion at best was only an anxious wish—a great perhaps!" and he replied that most men, no matter what views they held during their earlier life, grew anxious as they approached the final goal; and he quoted a remark made by Johnson to Boswell: "No rational man can die without uneasy apprehensions"; and added that he had seen in the Capucine Church in Rome, where he had been a few years ago, a tomb of a cardinal, and on it were the words, "*Pulvis et Umbra et Nihil.*"

I suggested to him that he should adopt a view of the future that I had found in *Kant's Kritik der reinen Vernunft.* Kant in this passage points out that no one may boast that he knows that God and a future life exist: our conviction on these matters is not a logical but a moral belief,

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[1] See Appendix A.

there can be no certainty, and as our belief rest on subjective grounds it is not right to say, it is morally certain that there is a God or that there is an hereafter, all that one is permitted to say is, " I myself am morally certain " ; one cannot say, " I am logically certain that there is a future life." He seemed taken with the idea, but did not continue the subject.

I quoted to him on another occasion the opinion expressed in a passage from Mill, and he agreed with it. The exact words I could not of course remember, but I have since looked up Mill's " Essay on Theism " for the words :

> The belief, however, in human immortality, in the minds of mankind generally, is probably not grounded on any scientific arguments either physical or metaphysical, but on foundations with most minds much stronger, namely on one hand the disagreeableness of giving up existence (to those at least to whom it has hitherto been pleasant), and on the other the general traditions of mankind. The natural tendency of belief is to follow these two inducements . . . . as causes of belief these various circumstances are most powerful. As rational grounds of it they carry no weight at all.

In another place Mill says : " What is called the desire of eternal life is simply the desire of life. There is, therefore, in science no evidence against the immortality of the soul but that negative evidence, which consists in the absence of evidence in its favour."

When Spencer published his " Autobiography " I was pleased to see that he had the same view on immortality as I had quoted to Tait from Kant, for he says : " As may be supposed, my position in respect to the question discussed was agnostic—the position that on the one hand there is no evidence supporting the belief in immortality, and that on the other hand there is no evidence to warrant denial of it."

In referring to Tait's observations that men who have passed through life holding the most positive views of the future, often falter as the journey gets to the end, I may quote two passages to illustrate how the most adamantine may find that they are not quite so sure as they were in their younger days.

Herbert Spencer, in his old age, wrote these words:

> After contemplating the inscrutable relation between brain and consciousness, and finding that we can get no evidence of the existence of the last without the activity of the first, we seem obliged to relinquish the thought that consciousness continues after physical organisation has become inactive. But it seems a strange and repugnant conclusion that with the cessation of consciousness at death, there ceases to be any knowledge of having existed . . . the consciousness that without origin or cause infinite space has ever existed, must ever exist, produces in me a feeling from which I shrink.

Huxley says: "It is a curious thing but I find my dislike to the thought of extinction increasing as I get older and nearer the goal."

During his visit to Rome in 1890, where he had gone to operate on the Czar's physician for gall-stones, he had been bidden to an audience with the Holy Father, who had consulted him on a matter which is of some moment to a surgeon if he happens to belong to the Roman Catholic Church. The point raised at the audience was: "Should the life of the fœtus be considered before the life of the mother?" The Roman Church holds that the life of the fœtus must be taken into consideration before the life of the mother, and Tait was consulted on the risk to the fœtus, and the risk to the mother, if an operation were performed on a child that had gone to term in an ectopic sac. I do not remember what views he expressed, but I am sure that he would have sacrificed a fœtus at any time to save the life of the mother: the teaching of the Roman Church on this point cannot be too strongly condemned.

Tait was much opposed to those members of the profession who deliberately committed abortion, whether for a fee or to oblige a friend. He very aptly quoted Seneca's words: "If what you do is criminal what matters it that no one else knows when you know it yourself."[1]

One day, in discussing the result that education had had on country squires, Tait remarked that Macaulay in his "History" had painted the English country gentleman of the

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[1] "Si turpia sunt quae facis, quid refert neminem scire, cum tu scias."

reign of Charles the Second, in such colours that it showed the country gentleman to be an ignorant drunken brute, but still having one virtue and one alone, and that a wonderful love and respect for one institute, and that the Church of England; and Macaulay points out that the experience of many ages proves that men may be ready to fight to the death, and to persecute without pity, for a religion whose creed they do not understand, and whose precepts they habitually disobey,

I made Tait laugh one day when I told him that in an examination a boy had once answered to the question: "What is faith?" "Believing what you know isn't so!" And he laughed very much when I told him that Charles Darwin's father had called Unitarianism "A feather-bed to catch a falling Christian."

* * * * *

And now I have come to the end of my task, and I have but little more to say.

Without detracting from the work of his predecessors I claim that the merit of establishing modern abdominal surgery belongs to Lawson Tait. But just as in considering the claims of Darwin as the man who placed the doctrine of organic evolution on a firm foundation, we must not neglect to pay a just tribute to the great geologists, Hutton and Lyell, for their preliminary labours, so must we acknowledge the pioneer work in ovariotomy done by McDowell, Atlee, Clay, Spencer Wells, Baker Brown, and Thomas Keith, and admit that just as the "Origin of Species" was the child of the "Principles of Geology," so Tait's work in abdominal surgery was the logical extension of the work done by his predecessors who dealt almost entirely with ovarian cysts.

I have pointed out how, from time to time, the early ovariotomists were led to attempt hysterectomy, because having opened the abdomen, thinking they had to deal with an ovarian cyst, they found they were face to face with a uterine myoma and sometimes proceeded to remove it, and occasionally these operations were successful.

In this way Spencer Wells, the man who had had most experience in the operation of ovariotomy, came to perform at odd times different abdominal operations with more or less success. Thus Spencer Wells was a true pioneer in abdominal surgery. Still, the fact remains that the removal of ovarian cysts, and the removal of myomata of the uterus constituted abdominal surgery up to the time when Tait extended its sphere by attacking in turn the gall-bladder, the liver, the ovaries, the Fallopian tubes, and the broad ligaments. Then it was that the work of the pioneers was crowned.

It was inevitable that the advances that were being made in general surgery through the introduction of Listerism would sooner or later extend to the abdominal organs; and I maintain, as I have said repeatedly in these pages, that when Tait discovered that he was able to return an ovarian pedicle, tied with a clean ligature, to the peritoneal cavity and abandon it with success, from that moment Tait began to seek for abdominal organs, other than the ovaries, upon which to operate, and thus it was that he came in the course of time to operate on diseased Fallopian tubes, and lay the foundation of the surgery of the pelvis, which soon became the starting-point and practice field for all who proposed to become abdominal surgeons in a wider sense.

Professor Judd, in writing of Lyell and Darwin and their labours in establishing *Evolution*, says:

> There is a tendency, when a great man has passed from our midst, to estimate his merits and labours with indiscriminating, and often perhaps exaggerated, admiration; and this excessive praise is too often followed by a reaction, as the result of which the idol of one generation becomes almost commonplace to the next. A still further period is required before the proposed position of mental perspective is reached by us, and a just judgment can be formed of the man's real place in history.

It is over twenty years since Lawson Tait died, and I think that a correct judgment can now be formed of his work. If some one had not come forward and given an account of his life work, the present generation might not

have taken the trouble to find out what this great pioneer in our art had done.

I have held a brief for my dead master, and I trust that I have been able to show in the preceding pages that Lawson Tait was one of the great pioneers in surgery, in as much as he was the real founder of Modern Abdominal Surgery.

* * * * *

"Close up his eyes and draw the curtain close;
And let us all to meditation."

HENRY VI. Part II.

APPENDIX A

The Image of Baal[1]

"He put away the image of Baal that his father had made."—2 Kings iii. 2.

I do not know any schismatic difficulty which has occurred in the whole history of the growth of the Christian religion, which has not had its origin in the unnecessary assumption of the plenary inspiration of the Scriptures, and even now, after countless indications of the human origin of the book have been admitted, and illustrations given of human errors contained within it, this tradition of the infallibility of its jots and its tittles is the great difficulty we have to contend against in our efforts to advance the cause of humanity. The great hindrance to our progress in true Christianity is our want of union; and it is not upon fundamental principles that we are disunited, but upon points not worth fighting for, upon letters of the law which are out of harmony with its spirit; and these make us desirous to pave the way to heaven rather with points of metaphysical subtlety than with the plain, broad, unmistakable truths upon which Christ based all His teachings.[2]

I have said that it is wholly unnecessary to assume plenary inspiration or inspiration at all. To the critics of Biblical literature I shall leave the question as to whether there is any evidence upon which such an assumption can be grounded. I shall attack it only on *à priori* grounds. We cannot suppose that the books of the Bible were meant to comprise the sum and substance of human knowledge, beyond which we were never to trespass or hope to acquire information; though we find that the Mahometans have accepted this view for the Koran, for they have said that all that man requires to know is therein contained, and what is not there is not worth knowing. And there is a logical consistency about their position which is worthy of our admiration. They evince in this way a more real and consistent respect for their Koran than we do for our Bible. If either of these books is entitled to the honour of plenary inspiration, it must be, like its author, complete and unchangeable.

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[1] An address delivered January 30, 1878.

[2] The work of the Revision Committee has shown already an abundant need for the correction of errors; and error once admitted, the only limit to its further discovery is the limit of just and learned criticism.

On the other hand we are bound to regard every truth in science, every fundamental law formulated irrevocably from the facts of nature, as a direct divine revelation ; for as all such are complete and unchangeable, their only source of origin must be the Great First Cause, the Almighty God Himself. Now we not only find these truths and laws of science not dealt with in the Bible, but in some very important particulars we find them entirely contradicted, and in this we have an incompatibility which is entirely subversive of the assumption of plenary inspiration. It is an unnecessary difficulty which, when removed, allows the Bible to be placed in its proper place, as an incomplete history of a remarkable people who have exercised an enormous influence on the history of the world, and also as a history of immense importance to the student of evolutionary philosophy.

The first great convert who was drawn by the doctrines of Christ was a man great in all he undertook. He infused into the half-hearted work of the disciples who had gone before him a large muscular earnestness which commands the utmost admiration, but with it he brought an amount of dogmatism amd metaphysical stubbornness which has been in certain directions not free from disadvantage. In reading the various opinions upon the works of Paul of Tarsus and the influence he has exercised upon the growth of the Christian religion, I am not at all surprised to find that very various estimates have been placed upon them. Some go so far as to say that he did as much harm as good to the cause he undertook, but with that I can hardly agree. We must bear in mind the times in which he lived and taught, the moral courage the man must have possessed, the difficulties which any one of the better Jew blood must have had in taking up a new, misunderstood, despised and persecuted creed. Brought up in a strict Pharisaic circle, nurtured in a school where the subtleties of the Greek sophistry abounded, he could not be expected to free himself from his early antecedents utterly, and pass into the broad clear lines laid down by the Master. The good he did in his own day must have been immense. Indeed, had it not been for his great broad figure and the support it gave to the tragedy upon Calvary, it is greatly to be feared that the martyrdom would have lost its effect upon the world, and would probably have faded into the obscurity in which are lost other events of a kind closely resembling it, and which we know of, only by faint glimpses in history. There can be no doubt that Paul's force made the Christianity of the first three centuries. Then came a long interval in which his influence was lost ; during which, in fact, the leading features of Christianity itself were lost, not from any fault of the system, nor perhaps much from the fault of those who were the teachers of its precepts, but because the facts of its discovery were not suited to the field upon which they were to be applied, the soil was not ready for the seed, and therefore it lay

dormant until it was turned over by the great ploughmen of the fifteenth and sixteenth centuries. These men, unfortunately as I think, but perhaps fortunately as some may think, and as it yet may prove to be, tumbled upon the implements which Paul had fashioned to their hands ; and thence arose that system of metaphysical hair-splitting, that long battle of the schoolmen, the war of the sects which has come down to us, and which rages even now, and in our own country, as bitterly almost as ever it did ; though, thanks in great measure to our want of tradition and to a great teacher now gone from amongst us, less in our own town than in any community with which I am acquainted. For these schisms and for this bitterness neither Christ nor His doctrines are in any way responsible. There is not one word in the whole of His recognised sayings upon which a single sectarian dogma can be built. But Paul, on the other hand, must be accredited with most of them. For the establishment of one doctrine in particular, Paul is to a large extent responsible, and it is one which has been very disastrous in retarding the evolution of religious life. It has checked the growth of brotherly love, the basis of the Christian religion. It has upset even the teachings of Christ Himself, by rendering them inapplicable even amongst ourselves.

Paul assumed that the theosophy with which he was surrounded, and which emanated from the schools of the Greeks, was a retrograde step. He had grafted upon his Hebrew monotheism a something of a higher kind, which he himself hardly understood, and with a want of charity, scarcely to be blamed, perhaps, but much to be regretted, he passed a verdict upon his compeers in his well-known verse, " Professing themselves to be wise, they became fools, and changed the glory of the incorruptible God into an image made like to corruptible man, and to birds and four-footed beasts and creeping things." Now we could hardly expect Paul to be a sociologist or a positive philosopher or to be able to look for the indications of facts in the facts themselves, and in them only. He of course felt that his new light placed him on a level infinitely above his surroundings, and his strong human nature was too much for his reason. But the mischief which his view of his contemporaries has created has been enormous, because his words were held to be inspired. If, however, he could have looked back with an unprejudiced eye upon the history of his own people, a history which he must have known, his strong logical discrimination would have shown him that the ideas of a deity, even amongst them, had gradually undergone a slow improvement—so that the God of the early Jews was by no means the God of the Jews of his own time—and that the polytheism of the Greeks was only a prelude to, and a phase of, the development of the purer monotheism to which the Hebrew philosophy was even then only tending. His doctrine, however, was accepted, and its inevitable consequences have been handed

down to us ; and it is only in this our own generation that we are at last throwing off the shackles which Paul forged for us. The want of harmony between this view and the actual facts of human history has brought about some most amazing efforts on the part of theological writers towards reconciliation, the most singular of which is one (characteristic of the German school) to the effect that the knowledge of God exists in us, and has always existed in us, of two kinds, one potential, the other real, the first of which in the majority of instances has remained stationary, but in a few has undergone development, whilst the other has been the subject of persistent retrogression. I cannot attempt to follow such scholastic subtleties. I would only say that this would seem to be an expression, in less precise form, of the Eastern doctrine of the Spirit of God and the Spirit of Evil battling in man for the mastery. Only one of Paul's theological critics, so far as I have found, boldly declares that the Apostle is wrong. Reiche alone has advanced the view that in this passage Paul has stated the sequence of events in the inverse order : that the deification of the powers of nature and individual things preceded monotheism, because all the conditions for the highest development of the religious feeling were wanting ; that the course of the development of humanity begins with the completest rudeness and proceeds to the gradual perfection of the inward as well as the outward life. This is exactly the conclusion to which all scientific research now tends ; but when it was first shadowed forth, nearly thirty years ago, it was met by the usual cry of being unscriptural, infidel and heretic, because it was assumed that the language of Paul and his views had been divinely inspired. The logical conclusion of those who followed Paul was, of course, to quote the words of one of them :

> That the whole heathen world justly lie under the condemning sentence of the divine law ; that to all is made in the works of nature a revelation so plain of the eternal power and Godhead of Jehovah, that nothing but a wilful and sinful perversion of the light which they enjoy can lead them to deny this great truth.

This writer forgets the other factor in the process. He forgets altogether that it is the circumstances under which light is displayed that determines its effect. To use a familiar and striking illustration gathered from one of the last of the many great triumphs of science ; all the parts upon which the telephone is constructed have been for very many years in our possession, but it was only by the labour and genius of one man that they were arranged in their proper order and placed in their proper light. Against the quotation I have just read let me give one more, drawn from another and, I venture to think, a higher school of theology. Writing about this very passage in the Epistle to the

Romans, Bishop Colenso has the following very remarkable views :

> Is the moral guiltiness of the offences of our African savages to be measured by the high standard of pure Christianity ? Again and again, in Zululand some wretch is knocked on the head as an umtakali, or evil doer, supposed, by certain occult practices, to plot against the lives of the King and Royal Family and others, just as wizards and witches were believed to do not long ago—perhaps are still believed to do—in some Christian lands. Are we able to measure the guilt in the sight of God of such matters, when we remember how short a time has passed since even in England, with all the light of Christianity, and all the help of learning, and all the benefits of English civilisation, such a truly good and pious man as Judge Hale could condemn a supposed witch to be burnt to death, and believe that in so doing he was acting according to the will of God, and not merely according to the law ? Such things as these we must leave to the Righteous Judge of all the earth, who will assuredly do what is right.

We now begin to understand why Colenso is right. Our knowledge of good and evil, of morality and immorality, is undergoing, like everything belonging to us, a slow process of evolution from the lower to the higher ; and generation after generation, as new light breaks upon them, puts away the images of Baal which their fathers had made.

Understand me aright. There is no absolute change in what is right, or what is, on the contrary, wrong ; no change in the nature or the attributes of the Godhead. It is in the light only where the change occurs. We see things in a clearer light ; we progress from the lower to the higher, from the darker to the brighter, from the less to the greater ; and what we formerly saw only as through a glass dimly, we now come to see as God—face to face.

This is what I should like to see recognised in positive theology, as opposed to the former metaphysical conception of the Almighty by which we have been so sorely oppressed. I know full well how difficult it is for those who have grown up in the cherished belief of their forefathers, to listen even with patience to views which are heterodox ; still more difficult is it to let the new usurp the place of the old. Brought up in a country where some of Paul's greatest mistakes have been elevated into the utmost importance, I found it no small matter to free myself from the traditions of the elders. Knox and his fellow Calvinists fixed upon my fellow countrymen the belief that God had pre-ordained from all eternity that some should be elected to eternal life, and that others should be excluded ; and as I grew to man's estate I found this impressed upon the habits and thoughts of all around me. I naturally reasoned that if I were one of the elect there could be no inducement for me to regard the precepts of Christ, when my inclinations of interest led me in another direction. On the converse, if

I should not be on the favoured lists, there was a similar absence of inducement for obedience to the moral law. In either case the destiny was not in my hands, and it mattered little what I did. To me, a God who could make such a profitless bargain, and one so detrimental to the interests of all concerned, could not occupy the temple; and therefore I put away the image of Baal which my fathers had made.

At the time my freedom was completed—and it of course was arrived at only by slow progresses—there came upon the world, like a new revelation, the first dawn of the evolutionary philosophy, and of that I became an ardent student; and it was not long before it became evident that if Mr. Darwin's discovery was to be applied with success to physical life and structure, it could be equally adapted to the moral and physical nature of man. If man's structure has been slowly evolved from lower stages, then so his conceptions of religion and morality, and his notions of the Godhead (not the Deity Himself, that has ever been the same, but the image we have made of Him) have been ever growing, ever changing with the increasing light which has been slowly opening upon us.

Let us now briefly trace the possibilities of the development of a conception of a deity from the material ever ready to our hand, the living beings around us, human and otherwise; and as no religion can exist without a deity, nor a deity without a corresponding religion of some sort, the development of such a conception may be held to be the evolution of religion.

If we analyse a religious sentiment, we find it composed of a process of reasoning which results in the comparison of two beings, or things, the result of which is the allowance that one is better than the other. This seems too simple to be fundamental, but it is quite enough for the hugest results.

Now we have long got past the stage at which humanity rested so long, and which allowed the process of reasoning to exist in the mind of man only; all the mental acts of the lower animals being relegated to the term "instinct." We now know quite well that animals reason, within certain limits and up to certain heights, as closely as man does. They reason upon events, and they record results. They can compare two things and decide which is the better. Thus, a dog after a few fights with other dogs, reasons upon the results of his experience, and generally records his conclusion that a bigger dog is a better dog, better able to fight and more likely to beat him, and therefore the lesser dog generally treats the bigger dog with respect and veneration. This is not always the case, for dogs are like men, and have not the religious conception equally developed. But you must all have seen, as I have very often, a little dog cringe and fawn, lie upon his back with uplifted paws, before a bigger dog. With his submission the emotion of fear has the chief effect, and this is the

primitive notion of all forms of religion. The savage does not respect anything as a god unless he first fears it; neither does the dog; neither, indeed, do a large number of civilised people. But if you watch the cringing dog at his devotions you may sometimes find him, when he has a favourable opportunity, give the object of his fear a bite. The savage whips his idol; and the mediæval Christian cheated his altar of its proper candles, giving "rascally dips," as Ingoldsby says, when he thought the saint might with impunity be imposed upon.

The dog recognises in man a superior being and worships him accordingly, the worship arising without doubt originally, and continuing to a large extent from fear, but affection ultimately enters also into the conception. But not only this; the dog's reasoning powers have so far advanced as to have formed in certain cases abstract notions upon this subject. I have, for instance, known a dog who recognised the implement with which he was punished, and it was quite enough to point to it hanging on the wall to produce a very manifest effect upon him in recalling him to a sense of proper behaviour; but one day, when he found the dreaded fetish at a disadvantage, he made off with it and gnawed it to pieces. Dogs have, therefore, well developed notions of right and wrong, punishment for ill-doing and reward for well-doing, and to this they have been brought by civilisation from the original wild condition in which their relatives, the fox, hyæna, and wolf, still remain.

The application of this is proved when we learn that we have human traces still existing, in the Juangs, the Andamanese, and others, whose notions upon such subjects are really by no means so far advanced. Amongst the first of these, the Juangs, there seems to be absolutely no trace of a religious conception of any kind, and yet we class them, structurally, as human beings. The earliest phase of religion in man is the acknowledgment of superiority in one man over another or others—that superiority being of course, as in the dog, of a physical kind. As the earliest form of government is of the patriarchal or family kind, the head of the family being its chief and ruler, the first religion necessarily is the reverence paid to the physical superiority of the parent by the child. In this way ancestor worship was established; and that, as we know, still remains the practical religion of a nation so numerous as to be said to constitute nearly a third of the human race.

The physical conception of a deity, therefore, at this stage is the record of the personality of the parent. When the parent dies the influence he exerted remains, and that being associated with his burial-place, or with his personal belongings, the idea arises that some of it may actually reside in these things, and the primitive conception of relic worship and tomb worship is the result, as again seen in the Chinese tablet worship, and their

services at the grave. Our own curious gravestones and lockets containing hair are survivals, undoubtedly, of this phase of our development.

The form of government advances from that of the family simply to that of the tribe, and then the conception of the deity enlarges. It is the chief man of a tribe who is deified, not only after death but often during life, as is constantly illustrated in the tribal communities of Africa and America. Some peculiarly powerful or successful chieftain lives in the memories of his people, and he becomes a more permanent deity—living in tradition, and his glories being added to by the continually increasing inventive power of his descendants. It was in this way that all primitive polytheisms were built up. Heroes were deified, and then the reasons of their deification were converted into abstractions, and the personal identity of the original was lost in the purer conception. This, for instance, was clearly the origin of a polytheism which at one time covered the whole of Northern Europe, and is generally known as the Scandinavian mythology. Its chief characters, Odin, Thor, and Freya, were all human celebrities deified. Mr. Herbert Spencer points this out clearly and gives modern instances, as in the case of the late Rajah Brooke, who has been deified by the Dyaks. Great Kings and chieftains caused themselves to be elevated into deities, as was commonly done by the Greek Kings and the Roman Emperors, and they substantiated their pretensions by a claim to divine descent. Thus, Mr. Spencer quotes from the Babylonian inscriptions, the claim of the well-known Nebuchadnezzar to be the direct creation of the god Bel. In the history of the Jewish theology this process of evolution is quite as well seen. The first conception we have of the deity in the Hebrew record is undoubtedly that he was a household god of Abraham, Isaac, and Jacob, interested only in their welfare, and in the well-being of their flocks and herds; and not only condoning, but originating and sanctioning all sorts of immoral schemes which had the one-sided object of forwarding their family interests. Then he advances a step and his views become wider. His concern embraces the descendants of the Patriarchs now gathered into a tribe. He watches over them, protects them miraculously, and by all sorts of unfair means defeats and shamefully ill-uses their enemies; their enemies being peaceful races, inhabiting luxurious countries which the chosen of the Lord have coveted. But sometimes this deity leaves his people in the lurch and forgets the covenant he has made with them. Then they punish him. They build altars to rival deities, erect golden calves, and build groves and temples to Baal. In fact, the whole history of the Jewish Kings is that of the struggle of a series of rival deities for mastery; a crude polytheism, from which a crude monotheism was to arise. We find one of the most curious

phases of it in the story of Ahab. He was in doubt as to which of the idols to serve, but he decided in favour of Baal, having no purer conception of any. So "he reared up an altar for Baal in the house of Baal, which he had built in Samaria. And Ahab made a grove and did more to provoke the Lord God of Israel to anger than all the Kings of Israel that were before him. . . . Then Jehoram, son of Ahab, put away the image of Baal that his father had made."

This god Baal, to whom such constant reference is made in the Bible, was, as we all know, the sun; and sun worship was one of the earliest, as it was also that most widespread, and is still the most persistent of all the systems of monotheism. How persistently remnants of sun worship remain even amongst ourselves perhaps few are aware. The common name of the first day of the week is the most familiar, but we have scores of others. The building of our churches to the East, the much-discussed question of the eastward position in certain parts of the service, the nimbus round the heads of the saints, the matin and vesper services, the peculiar arrangements of churchyards, and scores of other curious and apparently meaningless details, are all survivals of sun worship. It is not to be wondered at, therefore, that Baal had a permanent place in the Hebrew polytheism, and that even when the monotheism grew out of it Baal's burnt offerings and other remains of his former worship were retained as physical accompaniments of the purer conception which was slowly evolved by the Hebrew philosophy. At the time of the Birth of Christ, we find their conception had arrived at the phase in which a national deity had been evolved —a God of Israel and of Israel only, in whom the Gentiles had no share. No wonder, therefore, that they resisted the discovery of Christ, who lifted by one stroke of genius the monotheistic idea out of the narrow Jewish limits, and held up the God of the world—God of Jew and Gentile alike, and abolished for ever the parochial God of Judea.

But Paul could not see it in that light. He had still a survival of the old faith in him, and with Christ's words ringing fresh in his ears, he said, "for the Jew first, and then for the Gentile." He could not wholly put away the image of Baal which his fathers had made.

And now, how does it stand with us? We, at least those of us who need creeds, repeat week after week a declaration that there is one God, the Father Almighty, Maker and Ruler of all things, who is a just Ruler, a perfect Law-giver, and in whose sight all men are equal. But what we really believe, at least what we indicate as our real belief by our actions is, that this Almighty Being makes and unmakes laws upon some eccentric and unknown principle; that He favours some of His children to the detriment of others; that He is to be persuaded by much

talking; that He loves sackcloth and ashes; that He is to be hoodwinked by lists of subscriptions and the wearing of phylacteries; that the infraction of His laws can be made with impunity provided it be covered by the use of a prayer-house and a praying machine; and that most of all, He will especially damn and burn for ever all those who do not have quite the same image of Him in use as we have. We especially regard Him as the parish deity of Great Britain, and pray to Him not to go near the parish of Russia or France or America, or whatever spot of the earth's little surface happens to be at the time most hateful to us, but to leave it to our mercy, and let us wreak what desolation we may upon it; and we proceed to cut one another's throats to the glory of this image of Baal. And after we have had our little victory, we go to our little groves and our houses of Baal, and we set our little praying machines going, and call this the glory of God.

We have as yet failed to understand Christ's God, who is the God of the Universe, not of this little bit of it, or that. He is not the God of one sect or of one creed. He is the God of law and order, who needs no miracles for His glory and no offerings for His praise. His law is to be sought for with all our hearts and all our souls, and we should apply it to our neighbour exactly as we apply it to ourselves.

Even up to the present moment the popular conception of God is a mere anthropomorphism. It gets purer and purer as man advances, but it still retains more than it need do of the human phases through which it has passed. That the Great First Cause is absolutely unsearchable, who can say? Every revelation brings Him nearer and nearer to us; every fact unravelled and every law established is another page mastered in the great book of the mystery. Every age brings to us a purer and higher conception of the Godhead, and every advance in this the highest of all our efforts, brings us nearer and nearer to the time when there shall for ever disappear from amongst us the image of Baal which our fathers had made.

APPENDIX B

ORIGINAL COMMUNICATIONS

From the "Birmingham Medical Review," May 1899

The Bedstead is an Important Factor in Domestic Hygiene and Comfort

By Lawson Tait, F.R.C.S., M.D., LL.D., etc.

There is no article in ordinary lifelong use with which we are so closely related, with which we spend so much of our time, which we so often occupy with such affectionate readiness and leave with such poignant regret as our bedstead. Yet how little care is given, certainly but little was given in the past to the principles of its construction and use, so that personally I feel that everything between the primitive Scotch box-bed filled with a good elastic truss of well-dried heather and one of Whitfield's Ideal Bedsteads, introduced by that firm with my own name attached as a hospital bedstead, there is nothing to choose. Both of these have excellently well carried out the first necessities of health and comfort, and all between had better be given up.

This is the theme I propose to discuss in the following pages, combining in my narrative of the "bedstead as a factor in sanitation" some advice which I regard as wholesome and much needed in the general management and arrangement of our bedrooms. That this advice will be successful I have little doubt from the widespread revolution caused by a little paper which I wrote some twenty years ago, on "Bedroom Ventilation," out of which have grown the whole army of automatic ventilating and heating arrangements combined with and made into stoves of all kinds, gas and coal burning. Had I patented the suggestions in that paper, I had now been more than a millionaire, but I do not regret that I gave my plans free to the public as I now give my notions on bedroom management.

It is always useful and full of interest to trace the evolution of any object, whether in the animal frame or in the ordinary use of humanity—and the growth of the bedroom and the bedstead have their interest like other things. From the time of the cave dwellers through the various stages of savage and nomad life, the dwelling space was used for all purposes in common, and it is

only after the wandering hunter has achieved fixity of tenure and residence that he discovers the advisability of shutting off part of his house as a sleeping place. This becomes easier as he learns how to use the more permanent kinds of building materials, timber, brick, and stone; and as his skill grows, so does the fixity of possession and its continuity by descent, so that it comes practically that the bedroom appears with the pursuit of land tenure and culture and the erection of permanent houses. This brings us into the midst of advanced civilisation in which in Britain probably we were left by the Romans, the construction of whose bedrooms and bedsteads is quite well known, though neither were well adapted for permanent use in such a climate as ours. Even with the help of the efficient hypocaust the comfort of both in Britain must have been difficult to maintain, and when the skilled Italian artificers disappeared from our country in the fifth century we probably went back to the bundle of ling and its thin covering.

The Normans brought back a better kind of domestic building, and when the settlement of the country towards the end of the fifteenth century made domestic architecture possible for others than the Norman barons, the English yeoman adopted for his buildings the only material at his hand, the rough oak of his native woods and the daub and wattle still seen in the interstices of the beams till this day, formed from the osiers of the meadows and the mud from the river-side. The Norman nobility, as we know from old drawings and tapestry representations, used the single bedsteads as used on the Continent now. The castles in which they lived were massively built of stone by guilds of imported masons. The floors were level and the doors could be made quite square, and fitted fairly well. The windows were provided in the main rooms with well-fixed glass, and at other times with well-made oaken shutters, so that the chambers of a Norman, with their large fireplaces abundantly supplied with wood fuel, must have been exceedingly comfortable habitations. The enormous thickness of their walls secured a very fair uniformity of temperature, and the downfall of cold air from the walls was modified by the hangings of tapestry. For their bedsteads there was no need of hangings, and, so far as I can find out, they did not use them.

But the Saxon farmer had to build his dwelling on lines determined by the curves of the beams at his disposal. The quaint and picturesque twists of a fifteenth-century "oak and wattle" building are not the results of "settlement" or bad workmanship, and the inequalities of floor level were determined not by eccentricities on the part of the builder, but were the best he could do with his material. Casements, therefore, and doors could rarely be made true and closely fitting, in fact, they never were till late in the sixteenth century, when we find these beautiful panelings coming into use. "Draughts" in bedrooms must

have been therefore the rule and not the exception alike in castle and farmhouse. In the former they arose not only from badly fitting doors and casements, but from the downfall of masses of chilled air from the stone walls, and therefore tapestries were hung round the rooms to keep warm the occupants of the little Norman bedsteads. The farmers could not afford the costly products of the French looms, and their women had something else to do than spend their time over the tambour frame. Therefore a smaller apartment was constructed within the bedroom in the shape of a huge four-post bedstead, hung round with curtains, probably of common homespun serge, or the box-bed of the north was employed, though no trace of this has been found, so far as I know, south of the Humber. Into such contrivances a whole family must have crowded, probably with the addition of occasional visitors. This, of course, is an occurrence common enough even now in outlying Highland districts, where the box-bed with its complete set of shutters still is in common use. Such a marvel as the Great Bed of Ware, dating certainly from the reign of Henry VII., and perhaps earlier, measuring twelve feet square, represents the best instance of the "room within a room" which was still in use in the vast majority of well-to-do houses until the beginning of the series of "Great International Exhibitions."

Between 1851 and 1861, the French taught us the use of single bedsteads, and we introduced washhand basins and other lavatory arrangements to their notice, whilst the only permanent advantage either of the nations gained from the Crimean War was the use of the bath and bathroom, taught us by the Russians, widely used now in this country, not so widely in France. The Great Bed of Ware, still preserved in the old timber and wattle Rye House Inn at Hoddesdon, is traditionally said to have accommodated twelve men and their wives, and if we may judge from what Howard records early in this century on the customs in hospitals and workhouses of the Continent and this country as well, in certain places, the statement about our fifteenth-century carved oak bedsteads must not be regarded as any exaggeration, and we must not wonder at outbursts of plagues and pestilence arising directly from such causes. Nor does the moral atmosphere arising from such habits require explicit illustration.

The dawn of domestic sanitation, as we now know it, occurred as late as I have already said, and was not fully appreciated till well on in the 'seventies. Bath-rooms began to be added to our homes. We still had grates in our bedrooms, with "closing registers," so as to preserve that delicious "bedroom" odour familiar in all sleeping rooms badly ventilated, and which, in my belief, is the initial stage of the growth of the power of typhus fever. Four-post bedsteads were still in common use, but the curtains had become mere survivals, as seen in a valance along the

top rail and hangings about a yard wide at each corner, only sufficient to show what had been their original purport, but quite ineffective to keep off the draught which at that time was an essential feature of an English bedroom. The bed was a huge ungainly object, expensive and uncomfortable, fitted with wooden slats, which resisted all movement and exercised their uncomfortable influence through the thickest of mattresses and feather beds. It was, of course, made of huge slabs of wood, into the interstices of which vermin sought and obtained access, and once there they could hardly ever be exterminated.

I remember very well that in London it was, one might say, the rule to have vermin in all bedsteads used by the public, as in hotels and lodging houses, and private houses were by no means uniformly free from them. Bugs were the subject of chronic jokes in all humorous writings, whilst at the present advanced period of the development of sanitation they are never alluded to. This is one of the minor improvements, minor in the name perhaps only, which have taken place from the persistent arguments in favour of simple cleanliness, which is the gospel of true sanitation.

The first step in the right direction was the introduction of metallic bedsteads, and I remember very well the furor of indignation with which they were met—for nothing annoys an Englishman so much as any interference with his personal habits or belongings. Arguments of the most ridiculous and improper kind were used on all such occasions. Thus I have heard it urged by men who were otherwise sane, that they would not have gas fires because they could not spit in them, and could not poke them. But metal bedsteads have had their way, and now a home furnished with wooden bedsteads is looked upon with suspicion, and an hotel so provided would have but scant favour in popular estimation. But the old form of bedstead was imitated, of course, as closely as could be, just as the old chariot was imitated in our first railway carriages. The old wooden slats were replaced by a cross work of iron laths, as rigid and uncompromising as their predecessors. The "half tester" continued the survival of curtains, quite useless, but of great comfort to the English housekeeper, as unintelligible as that "blessed word Mesopotamia." Then the bedsteads must be double to hold two people, in spite of the warning lessons bestowed on every household every time a member of it had an attack of illness. The additional comfort obtained by every Englishman and woman on a visit to the Continent when they found in their bedrooms two snug little single bedsteads placed side by side, made no impression till about ten years ago, when a few venturesome islanders began to dare the breath of scandal by having separate beds. There can be no doubt that this was the reason why the improvement was resented, for to this day

the proof of the worst that can be circulated concerning a married couple is that "they occupy separate rooms." Separate beds was and is, to some extent still, regarded as almost as scandalous. Yet in all the best homes in our country each bedroom has attached to it a "dressing room," with a single bed in it, and by this a great increase in comfort and health is attained. Now that we know that consumption is a disease communicated from one to another by contact and breathing the air already breathed by the consumptive, the hygiene precaution of separate beds ought to receive some public recognition. For centuries the Italian physicians have taught the possibility of the disease spreading from husband to wife, and from one person to another, when a tainted and a healthy person have occupied the same bed. There are doubtless many other diseases of which the same is true.

Having now roughly indicated what a bedroom and bedstead should not be, let me try to show what they should be, and what they easily may be made; and that this is a matter of real urgency to personal health and comfort must come home to every one when it is remembered that from the beginning of life to its end we spend more than half our time, and its saddest moments, in our bedrooms and in our beds.

First of all, the capacity of a bedroom should be such as to allow of at least 2000 cubic feet for each occupant. It should have a flue of at least six inches in clear diameter, and this flue ought not to be in an outside wall, otherwise the chimney is sure to smoke when there is a fire, and it will fail as an upcast shaft at all other times. Nearly all smoky chimneys arise from the fact that they are built in an outside wall which is too thin. The room should if possible be warmed by a gas fire, as in this way only can a uniformity of temperature be absolutely maintained, and for very young and very old people nocturnal variations of temperature are extremely dangerous. But a gas fire is not an old coal grate filled with asbestos. A gas fire must be constructed for the use of gas on entirely special principles. The sashes of the windows should fit well and in every instance be provided with plate glass, for the reason that nothing assists the variation of nocturnal temperature like the employment of the common sheet glass of the jerry builder. In going over the magnificent hospital built lately by the Birmingham Guardians for the old and sick, I had only one adverse criticism to make, that the windows, properly placed on both sides of the large wards, had not been provided with plate glass. The difference in price in the two kinds of glass is far more than paid for in the difference in the coal bills incurred within the first two years to keep the wards sufficiently warm, for plate glass is a most efficient non-conductor of heat, and thin sheet glass is not nearly so efficient.

In all bedrooms the head of the beds (I assume now that in every case the beds are single, and when there are two that they are placed alongside each other) should have their heads close to an inside wall. If this cannot be arranged then the wall and the ceiling for a space corresponding to the bedstead should be covered with a thick serge, loosely attached to the wall, to prevent the cataracts of air chilled by the cold wall falling on the sleeper. The worst of colds are caught in this simple way in the small hours of the morning, and many a fatal senile bronchitis might be saved by this simple expedient. For the same reason the bed must not be placed between a badly fitting window and the flue or the doorway. For ventilation by far the best plan is the familiar plan of making a small place between the window middle bars by raising the lower sash about two inches against a fixed inner sill plate. In this way a gentle stream of fresh air is diffused over the ceiling and displaces the foul air.

For the perfect hygiene of the bedroom there are of course very many points of importance into which I cannot enter, as that the walls should be double, with at least two and a half inches of something between them, that the outer layer of bricks should be vitreous (pressed Philadelphian), that the doors and windows should fit accurately, and, *most of all*, that the jerry builder should be avoided like poison.

We come now to the important matter of the bedstead, and the only people who require special consideration are those over 6 ft. high, for whom extra length must be provided. Otherwise no bedstead need be more, and all ought to be 6 ft. 3 ins. in length, and no bedstead whatever, if constructed on the principles I intend to lay down, need be more than 3 ft. wide, save for a few unfortunate people who are on exhibition. The material must of course be some metal, of which the best is steel and the worst is brass. Steel or iron, with the beautiful black enamel now used by Messrs. Whitfield, tastefully set off with a clean white Marcella counterpane, makes the smartest and prettiest bedstead in any room, be it in cottage or palace, and it gives the least trouble ; but, of course, tastes differ in the matter of material. Both head and foot rails ought to be finished in a strong flat arch, an essential feature for the strength and smartness of the bedstead. For strength and steadiness the head rail must have four or five vertical bars fully secured into its arch and transom, so that the pillows are not pushed out by restless sleepers, whilst the arch of the foot rail, not more than 6 to 8 ins. high, should have one horizontal bar to prevent slipping of the mattress. The head rail and upper legs are framed firmly all in one piece, and so with the foot rail and lower legs, whilst the frame of the bedstead is a solid parallelogram and a permanent frame for the " Dominion Wire Mattress " by far the greatest improvement in modern bedstead-making.

The adjoining figures will show that this beautiful and unique contrivance is a linkage of steel wire in short pieces, bent and hooked so that every two pieces enclose an elongated parallelogram, save at the margins. If such arrangement were carried over space indefinitely it would mean that pressure at any point would radiate equally in all directions. But as the frame of the mattress has a length of twice its breadth the diminished longitudinal strain is supplemented by a series of strong spiral steel springs, which serve the additional purpose of giving a point at which detachment of the springs from their frames may be begun should it ever become necessary to remove them, or if detached the process of re-attachment ends there. But there never ought to arise any necessity for detaching the springs or wire meshes, for the material supplied by Messrs. Whitfield is so good, the balances of tension so perfectly contrived, that it never varies. I am far beyond the average weight of male mankind, and I have used the same "Lawson Tait Bedstead" for now nearly seventeen years, and the tension of its springs has never varied in the slightest degree. The spring mesh is tightly secured all round the inside of the firm metal frame, made in one piece, so that the whole bedstead, weighing about 80 lbs., and joined together by bevelled key joints or dovetails, comes into pieces by a few smart strokes from below, upwards delivered on the dovetails on each corner, and for removals these three pieces pack on one another flat, and require only to be tied together. Compare this with the awful work of removing an old-fashioned four-poster.

Another of the faults of the old four-posters was that they were far too high, requiring, as we see in Hablot K. Brown's illustrations of Pickwick, wooden step ladders by which to mount into the apartment. This fault is continued, to the great discomfort of short people and those who are old, feeble and ill. Measuring the bedsteads of a number of first-class hotels I find that to the upper edge of the mattress, 30 inches and over is the usual height—far too much for any save people much above the average stature. Twenty-four inches to the upper edge of the mattress is far more comfortable for everybody, and the most comfortable bed, in my opinion, in the matter of height, is one on which the occupant can sit with the knee bent at very little more than right angles. For men of average height (5 ft. 8 ins.) this will be found to be about 17 inches, or just the height of an ordinary chair, and this should include mattress and bedding. The legs of such a bedstead would not be much more than a foot long, and the great advantage of this height with a width of 36 inches will be found in cases of illness in heaving people who require much moving, and this is the greatest test to which a bedstead can be put, for the attending nurses of the present day average only 5 feet 3–4 inches.

It was this matter of the bedstead during illness, especially surgical ailments, which first forced the question of the bedstead

and its proper construction upon my notice. In cases of illness the nurses are fearfully handicapped by an old-fashioned high double bedstead. The number of serious illnesses acquired by young women and caused by the strain of lifting heavy and helpless invalids, has been very large in my experience. Therefore I say have all your bedsteads single, narrow, low, perfectly rigid, so constructed that they will not sag, and have a stout steel screw-eyelet fixed in the ceiling over the site of every bedstead, into which a cord may be fixed when wanted.

This question of sagging of a bedstead is of the greatest importance, and a thoroughly depressed bed-frame is a trouble even to those who are well, whilst in illness it means inevitably that however well placed the patient will shortly work round so as to be on his back—always a helpless position, and one which often contributes greatly to the discomfort of a patient, and sometimes even to his death. I have completely cured sagging by throwing the top and bottom rails of the mattress-frame into a widely opened arch, so that when unoccupied the whole wire-mattress is slightly arched upwards from side to side, and when occupied by an average weight the mesh is exactly flat. Turning from side to side in such a bed is a matter of ease for even the feeblest, and the inevitable turning on the back is entirely prevented. The ring in the ceiling, which is no eyesore in the private bedroom, assists most materially in the movement of those even who are in acute illness and who without it would hardly move at all, and would require every movement to be made for them by nurses. We never know when a bedstead may have to bear a bed of illness, therefore the ring should have a very common presence in the bedroom ceilings.

Messrs. Whitfields have brought the " Lawson Tait Bedstead " to what I think is final perfection by the indiarubber foot pad. It prevents noise and jarring when the bed has to be moved, and it is an enormous advantage over the old castor which was noisy and frequently came off, and these were a source of much trouble. Thus the castor, whether off or on, was a potent factor for the marking and disfiguring of a well-faced floor.

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